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Edited for the
Academy of Political Science
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of Columbia University

The Social Origins of the American Revolution: An Evaluation and an Interpretation

JACK P. GREENE

Theories of Revolution in Contemporary Historiography

PEREZ ZAGORIN

The Gamut from A to B: The Image of the Black in Pre-1915 Movies

DANIEL J. LEAB

The Consequences of Corruption: The Ghanaian Experience

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Revolution and Growth: Two Case Studies from India

THOMAS A. TIMBERG

Reviews

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Professor Sigmund Diamond edited this issue of the *Quarterly*. The Board of Directors of the Academy wish to take this occasion to express its appreciation for his many years of service as editor.

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The Social Origins of the American Revolution: An Evaluation and an Interpretation*

JACK P. GREENE
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Given the rich theoretical literature on revolution that has been published over the last decade with its heavy emphasis on the relationship between revolutions and the social systems in which they occur,[1] it is hardly surprising that historians of modern political revolutions have increasingly turned their attention to the wider social context in their search for an explanation for those events or that their discussions of the causal pattern of revolutions now give as much attention to social strain as to political and ideological conflict; to social dysfunction, frustration, anomie, and their indices as to weaknesses and tensions within the polit-

* An earlier version of this paper was presented at San Diego State College and San Jose State College on March 20 and 21, 1972 as the second annual lecture of the The California State University and Colleges Statewide Lecture Series to Honor the Bicentennial of the American Revolution.

[1] Especially Chalmers Johnson, *Revolutionary Change* (Boston, 1966). But see also Neil J. Smelser, *Theory of Collective Behavior* (New York, 1962); James C. Davies, "Toward a Theory of Revolution," *American Sociological Review*, XXVII (1962), 5-19; Barrington Moore Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966); Henry Bienen, *Violence and Social Change* (Chicago, 1968); and Ted Robert Gurr, *Why Men Rebel* (Princeton, 1970).

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ical system.[2] This enlarged frame of inquiry has yielded a much more comprehensive understanding of several specific revolutions.[3] The question of whether it will prove equally fruitful for all varieties of great modern political revolutions is raised by several recent efforts to apply it to the American Revolution.

I

The idea that the American Revolution may have had a social content—that is, social origins and social consequences—is hardly a new one. It has existed in a crude form ever since the Revolution, and between 1900 and 1930 a number of individual scholars—especially Charles H. Lincoln, Carl L. Becker, Arthur M. Schlesinger Sr., Charles A. Beard, and J. Franklin Jameson—formulated an elaborate social interpretation of the Revolution.[4] Investigating various aspects of the internal political life of the American colonies between 1760 and 1790, they discovered what they took to be serious economic and social antagonisms underlying the major events of the American Revolution at virtually every stage of its development. Far from having been simply a war for independence from Great Britain, the American Revolution—as these men came to see it—was a *social* struggle by underprivileged groups against the special privileges and political dominance of the old colonial aristocracy.

The details of this interpretation are too well known to require lengthy discussion here. Colonial society by the middle decades of the eighteenth century was everywhere fraught with severe class

[2] See, for instance, the influential translation of the recent theoretical literature for historians by Lawrence Stone, "Theories of Revolution," *World Politics*, XVIII (1966), 159-76.

[3] Two splendid examples are by J. W. Smit, "The Netherlands Revolution," and by Lawrence Stone, "The English Revolution," in Robert Forster and Jack P. Greene, eds., *Preconditions of Revolution in Early Modern Europe* (Baltimore, 1970), 19-108.

[4] Charles H. Lincoln, *The Revolutionary Movement in Pennsylvania, 1760-1776* (Philadelphia, 1901); Carl L. Becker, *History of Political Parties in the Province of New York, 1760-1776* (Madison, 1909); Arthur M. Schlesinger Sr., *The Colonial Merchants and the American Revolution, 1763-1776* (New York, 1918); Charles A. Beard, *An Economic Interpretation of the Constitution of the United States* (New York, 1913); J. Franklin Jameson, *The American Revolution as a Social Movement* (Princeton, 1926).

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conflict arising out of an ever greater concentration of wealth into the hands of the privileged few and an ever more rigid social structure, an increasingly aristocratic political system, and severe resentment among the masses who were more and more being deprived of any opportunity to realize their own economic and political ambitions. Not surprisingly, the resulting tensions among these antagonistic social groups came surging to the surface during the Revolutionary controversy between 1765 and 1776 and, as Merrill Jensen later argued, converted the Revolution from a simple movement for independence from Great Britain into a "war against the colonial aristocracy."[5] Assessing the results of this "internal revolution," Jameson explicitly argued that the American Revolution profoundly altered many features of colonial social and economic life and represented a significant advance toward a "levelling democracy" in the Revolutionary era.[6]

In origins as well as in results, the American Revolution thus came to be thought of as the product not merely of a quarrel between Britain and the colonies but also of deep fissures within colonial society and as a social upheaval equal to the French, if not the Russian, Revolution. Because of its obvious indebtedness to the liberal concept of American public life during the era from which it emerged, this view of the Revolution has subsequently been labeled the Progressive interpretation.[7] But it was elaborated by still other scholars during the 1930s and 1940s. So widespread was its acceptance that it continued well into the 1950s to be the orthodox interpretation of the Revolution.[8]

As is well known, this whole interpretation was shattered and deeply discredited by the research of a wide range of scholars between 1945 and 1965. A number of detailed studies of segments and aspects of the political life in virtually every colony as well as

[5] Merrill Jensen, *The Articles of Confederation: An Interpretation of the Social-Constitutional History of the American Revolution, 1774-1781* (Madison, 1940), 11

[6] Jameson, 18.

[7] A penetrating general discussion of the three leading Progressive historians is by Richard Hofstadter, *The Progressive Historians: Turner, Beard, Parrington* (New York, 1968). For a more detailed analysis of the Progressive interpretation of the Revolution, see Jack P. Greene, *The Reappraisal of the American Revolution in Recent Historical Literature* (Washington, 1967), 7-17.

[8] Greene, *Reappraisal*, 13-17.

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many of the concrete social changes emphasized by Jameson seemed to indicate that, far from being similar to the French Revolution, the American Revolution was a peculiarly American event in which there had been remarkably little social discontent expressed, no real social upheaval, and relatively few changes in the existing American social structure. Thus, in the decades after the Second World War, the American Revolution came to be interpreted as an event that had been almost entirely political with little specific social content and that could be understood, therefore, almost exclusively in political terms. Where the Progressive historians had gone wrong was, first, in reading the fundamental social conflicts of their own day back into the Revolution and, second, in using a conceptual framework derived from the French Revolution and later European revolutions to identify the questions they brought to the American Revolution—questions, the data seemed to indicate, that were obviously irrelevant to the American experience.[9]

Predictably, and happily, we had just begun to feel comfortable with this view of the origins and nature of the American Revolution when the new analysis itself came under assault. The most compelling call for a revival of interest in the social content of the Revolution came from Gordon S. Wood in 1966 in an extended analysis of Bernard Bailyn's book-length introduction to *Pamphlets of the American Revolution 1750-1776*.[10] Commenting on Bailyn's heavy emphasis upon the role of ideas in the coming of the Revolution, Wood argues that it is "precisely the remarkable revolutionary character of the Americans' ideas now being revealed by historians [such as Bailyn] that best indicates that something profoundly unsettling was going on in society." The "very nature" of the colonists' rhetoric, "its obsession with corruption and disorder, its hostile and conspiratorial outlook, and its millennial vision of a regenerated society," Wood argues, reveals "as nothing else apparently can the American Revolution as a *true revolution* with its sources lying deep within the social structure. For this kind of frenzied rhetoric," he contends, "could spring on-

[9] *Ibid.*, 17-79, discusses in detail the implications of post-World War II scholarship on the Revolution. A representative collection of this scholarship will be found in Jack P. Greene, ed., *The Reinterpretation of the American Revolution, 1763-1789* (New York, 1968).

[10] Bernard Bailyn, introduction to *Pamphlets of the American Revolution 1750-1776* (Cambridge, Mass., 1965-), I.

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ly from the most severe sorts of social strain."[11]

As yet, Wood's call for "a new look at the social sources of the Revolution"[12] has not been answered by any systematic or comprehensive assault upon this problem. But there has been a wide assortment of specialized studies of various aspects of American social life between 1725 and 1775. Most of them have been, at the most, only implicitly concerned with explaining the American Revolution. But the data and conclusions they have provided may be used to construct and to evaluate some hypotheses about the possible relationship between social strain and the origins of the Revolution. The findings of these studies seem to lend themselves to one or the other of two central hypotheses. The first, which has never been explicitly formulated but has been gradually taking shape over the past fifteen years, is that colonial society underwent a dramatic erosion of internal social cohesion over the period from 1690 to 1760. The second, an updated and more sophisticated variant of the old Progressive thesis, is that over the same period, the social structure of the colonies was becoming more and more rigid and social strain correspondingly more intense.

II

The most impressive support for the first of these hypotheses is found in Richard L. Bushman's penetrating *From Puritan to Yankee: Character and the Social Order in Connecticut, 1690-1765*,[13] a case study of the various strains created by the rapid economic and demographic growth of that colony. But many other studies can also be used for its elaboration. According to this hypothesis, a great variety of developments combined to keep colonial society in a state of perpetual and disorienting ferment, a state that was both rapidly accelerating and increasing in intensity and extent in the decades just prior to the Revolution. Chief among these developments were, first, the rapid territorial and demographic expansion, including the influx of many previously unrepresented religious and ethnic groups, and, second, the extraordinary accel-

[11] Gordon S. Wood, "Rhetoric and Reality in the American Revolution," *William and Mary Quarterly*, XXIII (1966), 26, 31 (italics added).

[12] *Ibid.*, 24.

[13] Richard L. Bushman, *From Puritan to Yankee: Character and the Social Order in Connecticut, 1690-1765* (Cambridge, Mass., 1967).

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eration of the economy with an attendant increase in economic opportunity and abundance. But there were many other developments that contributed to the ferment: (1) the shattering of religious uniformity by that great spiritual upheaval known as the Great Awakening;[14] (2) the rise of towns and small cities with life styles and social dynamics that pointedly distinguished them from the traditional rural communities of colonial America;[15] (3) a quickening of the pace of social differentiation marked by the appearance of an increasingly more complex institutional and social structure and greater extremes in wealth;[16] (4) the intensification of the seemingly endemic factionalism of colonial politics as men competed vigorously and ruthlessly with one another in their quest for land, wealth, status, and supremacy.[17]

[14] See especially, Alan Heimart, *Religion and the American Mind: From the Great Awakening to the Revolution* (Cambridge, 1966); C. C. Goen, *Revivalism and Separatism in New England, 1740-1800: Strict Congregationalists and Separate Baptists in the Great Awakening* (New Haven, 1962); Edwin Scott Gaustad, *The Great Awakening in New England* (New York, 1957); William G. McLoughlin, *Isaac Backus and the American Pietistic Tradition* (Boston, 1967), 1-109; and Bushman, 147-232. All of these works concentrate on New England. For the impact of the Great Awakening elsewhere one must still turn to two older works: Wesley M. Gewehr, *The Great Awakening in Virginia, 1740-1790* (Durham, N.C., 1930), and Charles Hartshorn Maxson, *The Great Awakening in the Middle Colonies* (Chicago, 1920).

[15] The social impact of urbanization has not been systematically analyzed, but see the older study by Carl Bridenbaugh, *Cities in Revolt: Urban Life in America 1743-1776* (New York, 1955), and three recent studies of individual towns: Sam Bass Warner Jr., *The Private City: Philadelphia in Three Periods of its Growth* (Philadelphia, 1968), 3-45; George C. Rogers Jr., *Charleston in the Age of the Pinckneys* (Norman, Okla., 1969), 3-115; and G. B. Warden, *Boston 1689-1776* (Boston, 1970).

[16] On the process of social differentiation, see, among other studies, Lawrence A. Cremin, *American Education: The Colonial Experience, 1607-1783* (New York, 1970), 271-571; James A. Henretta, "Economic Development and Social Structure in Colonial Boston," *William and Mary Quarterly*, XXII (1965), 75-92; Kenneth A. Lockridge, "Land, Population and the Evolution of New England Society, 1630-1790," *Past and Present*, No. 39 (1968), 62-80; James T. Lemon and Gary B. Nash, "The Distribution of Wealth in Eighteenth-Century America: A Century of Change in Chester County, Pennsylvania, 1693-1802," *Journal of Social History*, II (1968), 1-24; and Jackson Turner Main, *The Social Structure of Revolutionary America* (Princeton, 1965).

[17] Bernard Bailyn, *The Origins of American Politics* (New York, 1968), 55-161.

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Some of the potentially corrosive effects of this ferment have been noted by a number of scholars. The movement of people from older communities to newly opened regions greatly weakened the ties of authority or community.[18] Extremely high rates of upward social mobility threatened the standing of traditional elites by devaluing their status and confronting them with a seemingly endless series of challenges to their monopoly of social and political power. At the same time, their reluctance to share authority with new men created severe status inconsistencies in which the new men had wealth—the main attribute of elite status in the colonies—minus the political power and social position to go with it. To the traditional elite, the movement toward a more inclusive leadership structure appeared to be too rapid, while to the new men it seemed to be too slow. Moreover, as new men challenged traditional leaders or as elites splintered in their bitter contests for political supremacy, the political and social standing of the elite as a whole as well as its internal cohesion was seriously undermined.[19] In addition, the settlement of new areas put severe strains upon existing political structures at both the local and provincial levels, as older centers of power sought to extend their authority over distant areas with economic interests, social and political orientations, and ethnic and religious compositions that diverged sharply from those of the older centers. Those strains sometimes—in North Carolina, New Hampshire, and New Jersey in the late 1740s and early 1750s; in Pennsylvania from the late

[18] On this point, see Bushman, 41-82, and Philip J. Greven Jr., *Four Generations: Population, Land and Family in Colonial Andover, Massachusetts* (Ithaca, 1970), 175-258. The character of life in newer communities may be surmised from Charles E. Clark, *The Eastern Frontier: The Settlement of Northern New England, 1610-1763* (New York, 1970), 111-267; Charles S. Grant, *Democracy in the Connecticut Frontier Town of Kent* (New York, 1961); and, for a very different area, Robert W. Ramsey, *Carolina Cradle: Settlement of the Northwest Carolina Frontier 1747-1762* (Chapel Hill, 1964).

[19] See Bushman, 41-134; Edward Marks Cook Jr., "Social Behavior and Changing Values in Dedham, Massachusetts, 1700 to 1775," *William and Mary Quarterly*, XXVII (1970), 546-80; John J. Waters Jr., *The Otis Family in Provincial and Revolutionary Massachusetts* (Chapel Hill, 1968), 76-161; Heimert, 27-236; and Jack P. Greene, "Changing Interpretations of Early American Politics," in Ray A. Billington, ed., *The Reinterpretation of Early American History: Essays in Honor of John Edwin Pomfret* (San Marino, Calif., 1966), 151-84.

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1740s through the mid-1760s; and in both Carolinas in the late 1760s and early 1770s—resulted in the breakdown of government or even in open sectional conflict.[20]

Nor are these by any means all of the examples of the severe destabilizing effects of the rapid changes taking place in the colonies during the middle of the eighteenth century. The Great Awakening unleashed and mobilized widespread discontent with existing religious establishments from New England to Virginia. From this discontent emerged a militant evangelicalism that rejected many aspects of the traditional social as well as religious order of the colonies and demanded sweeping changes in the relationship between church and state.[21] Rapid urbanization also produced some unsettling effects. For one thing, it led to the appearance of an urban-rural dichotomy in many colonies. More important, by crowding together in a small area large numbers of not fully assimilated and sometimes impoverished people, it created for the first time in the colonies a potential for the emergence of intraurban class and group antagonisms within the cities themselves.[22] It might also be argued that the process of institutional differentiation that was taking place within medium-sized and

[20] Greene, "Changing Interpretations," 159-72; Lawrence F. London, "The Representation Controversy in Colonial North Carolina," *North Carolina Historical Review*, XI (1934), 255-70; Donald L. Kemmerer, *Path to Freedom: The Struggle for Self-Government in Colonial New Jersey 1703-1776* (Princeton, 1940), 187-236; Brooke Hindle, "The March of the Paxton Boys," *William and Mary Quarterly*, III (1946), 461-86; Richard M. Brown, *The South Carolina Regulators: The Story of the First Vigilante Movement* (Cambridge, 1963); Hugh T. Lefler and Paul Wagar, eds., *Orange County—1752-1952* (Chapel Hill, 1952); and M. L. M. Kay, "An Analysis of a British Colony in Late Eighteenth Century America in the Light of Current American Historiographical Controversy," *The Australian Journal of Politics and History*, XI (1965), 170-84

[21] Heimert, 1-236; Bushman, 147-232; and J. M. Bumsted, "Religion, Finance, and Democracy in Massachusetts: The Town of Norton as a Case Study," *Journal of American History*, LVII (1970-71), 817-31.

[22] In the absence of specific studies of the social impact of urbanization, see Warden, *Boston*, 102-73; Henretta, 75-92; Robert J. Taylor, *Western Massachusetts in the Revolution* (Providence, 1954); Richard D. Brown, *Revolutionary Politics in Massachusetts: The Boston Committee of Correspondence and the Towns, 1772-1774* (Cambridge, 1970); Robert Zemsky, *Merchants, Farmers, and River Gods: An Essay on Eighteenth-Century American Politics* (Boston, 1971), 255-60, 264, 281; Warner, 3-21; G. B. Warden, L'urbanisation Americaine avant 1800," *Annales*, 25e année (1970), 862-79; James T. Lemon, "Urbanization and the Development of Eighteenth-Century Southeastern Pennsylvania and Adjacent Delaware,"

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large urban communities contributed to the attenuation of the traditional highly personal pattern of social relations by establishing institutional barriers between groups and thereby helping to bring about a significant depersonalization of the social system.[23] Finally, by whetting the social and economic appetites and increasing the material aspirations of men at all levels of society, the "profuse abundance" of the American environment seemed to have produced, in direct violation of inherited norms, a disturbing rise in several forms of hedonistic and anomic behavior and a corresponding decline in moral standards and devotion to the public good. What was even worse, at least in some quarters, such forms of behavior seemed to be gaining public acceptance, thus endangering the normative cohesion of colonial society.[24]

If the first hypothesis emphasizes the unsettled and chaotic state of colonial society during the mid-eighteenth century, the second argues that it was becoming too settled. Several case studies of communities as diverse in size and character as Boston and Chester County, Pennsylvania, strongly suggest that during the decades just prior to the Revolution, opportunity was declining and the social structure becoming less open in the older settled communities as a result of overcrowding brought on by a shortage of land, increasing social stratification, a greater concentration of wealth in the hands of the upper classes, rising numbers of

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William and Mary Quarterly, XXIV (1967), 501-42; Richard A. Ryerson, "The Revolutionary Committees of Philadelphia, 1774-1776: A Study in Crisis Politics" (Ph.D. diss., The Johns Hopkins University, 1972); Pauline Maier, "The Charleston Mob and the Evolution of Politics in Revolutionary South Carolina 1765-1784," *Perspectives in American History*, IV (1970), 173-96; and Carl Bridenbaugh, *Myths and Realities: Societies of the Colonial South* (Baton Rouge, 1952), 54-196; and Rogers, 3-88.

[23] There has been almost no attention to this kind of problem in colonial America, but see the extremely suggestive analysis in Peter Laslett, *The World We Have Lost* (New York, 1965), 1-80, 150-99.

[24] Jack P. Greene, "Search for Identity: An Interpretation of the Meaning of Selected Patterns of Social Response in Eighteenth-Century America," *Journal of Social History*, III (1970), 189-220; Perry Miller, "From the Covenant to the Revival," in James Ward Smith and A. Leland Jamison, eds., *The Shaping of American Religion* (Princeton, 1961), 322-68; Edmund S. Morgan, "The Puritan Ethic and the American Revolution," *William and Mary Quarterly*, XXIV (1967), 3-43; and Gordon S. Wood, "Rhetoric and Reality," 24-32, and *The Creation of the American Republic, 1776-1787* (Chapel Hill, 1969), 1-24.

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poor, and a pronounced tendency toward political elitism. All of these developments, the argument runs, created deep frustrations for those who found opportunity constricting and their life prospects growing correspondingly dimmer. The supposed result was the creation of severe underlying tensions between the privileged and the unprivileged, landed and landless, masters and servants, even fathers and sons.[25] These studies are still too few to know whether the phenomena they describe were widespread. But Jackson Turner Main in his more comprehensive investigation *The Social Structure of Revolutionary America* has speculated about the existence of a general "long-term tendency . . . toward greater inequality, with more marked class distinctions throughout the colonies on the eve of the pre-Revolutionary disturbances."[26]

III

Obviously, an enormous amount of further research will be required before we will know with some degree of certainty to what extent either or both of these two general hypotheses—the one emphasizing the destabilizing effects of rapid change and the other the frustrations created by a closing society—accurately represent some of the realities of mid-eighteenth-century colonial social development. On the basis of present knowledge, however, it is clear that at least three major questions need to be confronted.

The first is simply how to resolve a number of important ambiguities about the meaning and operation of several of these specific components of social strain we have just described. For one thing, there is an apparent contradiction between the two general hypotheses. How could colonial society have been becoming less coherent and more rigid at the same time? This contradiction might of course be explained in one of three ways. First, it is possible that there were significant spatial variations, that there was a tendency toward less coherence in some rapidly developing areas and more rigidity in other older and more stable areas. A less likely possibility is that a generalized long-term linear process was at work

[25] See, especially, Kenneth A. Lockridge, *A New England Town. The First Hundred Years: Dedham, Massachusetts, 1636-1736* (New York, 1970), 139-85, and "Land, Population and the Evolution of New England Society," 62-80; Henretta, 75-92; and Lemon and Nash, 1-24.

[26] Main, 286-87.

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with a period of intense social and economic upheaval being followed just before the Revolution by a time of declining opportunity and greater stratification. Finally, it is possible, as P. M. G. Harris has recently suggested,[27] that this process was cyclical, with colonial society becoming more or less flexible according to population change, community growth, institutional development, and various contingency factors, though the cycles may not have been quite so regular as Harris has posited.[28] My own suspicion is that some combination of the first and third possibilities will probably turn out to provide the most plausible resolution to the problem.

But there are many other more specific problems with each of these hypotheses, especially with the first. Like many of the works which can be cited in its support, this hypothesis rests upon an obvious, though unstated, and relatively crude application of an equilibrium theory of society. As critics of such theories have pointed out, to the extent that equilibrium theories, first, assess the health or sickness of society in terms of whether or not the elements within it are in a condition of homeostasis and, second, view change as an essentially disequilibrating force, such theories are potentially distorting, for it is not at all clear that the invariable price of rapid change is social instability. Obviously, change can be so well institutionalized and so widely accepted as to produce stabilizing effects. Indeed, in the specific situation at hand, it can be argued that change—and increasingly rapid change at that —was at the heart of the colonial life experience and that the colonists had been forced almost from the beginning to come to terms with it. Moreover, the second hypothesis suggests that the slowing down of change rather than its acceleration created a potential for serious social disruption.

More particularly, it is not yet at all clear that the cost of rapid economic and demographic growth in combination with increasing social differentiation was always a loss of social cohesion; certainly, although there was everywhere an acceleration of the economy

[27] P. M. G. Harris, "The Social Origins of American Leaders: The Demographic Foundations," *Perspectives in American History*, III (1969), 159-344.

[28] See the criticisms made by Daniel Scott Smith, "Cyclical, Secular, and Structural Change in American Elite Composition," *ibid.*, IV (1970), 351-74.

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and the population, it does not appear that there was a movement from more to less social coherence in most of the colonies during the first three-quarters of the eighteenth century. In New England, where the early settlers were reasonably homogeneous and for most of the seventeenth century at least generally devoted to an explicit religious and "social synthesis" that had been worked out by the leaders of the first settlements, this thesis seems to work fairly well, and it is significant that most of the evidence for it comes from New England. But the colonies to the south, the provinces from New York to Georgia, had been settled by disparate groups and individuals with widely divergent backgrounds, goals, interests, and orientations; and the direction of social development there during the eighteenth century was clearly toward more rather than less coherence and homogeneity as the settlers fashioned out of diverse materials a social synthesis where none had been before.[29] Even with regard to the New England colonies, it may be argued that the breakdown of the old Puritan synthesis early in the eighteenth century was accompanied by the emergence of a new one that, while retaining a distinctly Puritan flavor, was not so dissimilar in form and character from those achieved at the same time by the colonies farther south.[30]

And there are still other problems. One arises out of the uncertainty that the rapid upward social mobility, the accelerated movement toward political inclusion, or the Great Awakening functioned as destabilizing rather than stabilizing forces or that the kinds of conflict they engendered did not relieve as much social strain as they created. In addition, it is also possible that the widespread expression of concern about rising standards of consumption in the colonies was less an indication of fundamental discontent with existing modes of behavior than an appropriate —and necessary—device through which the colonists could fulfill an apparent need to preserve the *ideal* of a more static, coherent, goal-oriented, and instrumental social order without requiring them to alter their actual behavior.

[29] The point is obvious, but see, for example, Patricia V. Bonomi, *A Factious People: Politics and Society in Colonial New York* (New York, 1971); Gary B. Nash, *Quakers and Politics: Pennsylvania, 1681-1726* (Princeton, 1968); and M. Eugene Sirmans, *Colonial South Carolina: A Political History 1663-1763* (Chapel Hill, 1966).

[30] See, for instance, the intriguing suggestions in Zemsky, 69-74.

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There are far fewer obvious problems with the second hypothesis. But it is entirely possible that, in terms of opportunity and career chances for individuals throughout colonial society, the shortage of land in older communities may have been more than offset not only by the opening up of new areas but also by new opportunities in nonfarm occupations created by the ongoing process of social differentiation and institutional development.[31] Moreover, from the evidence so far adduced, it is by no means certain that the decline in opportunity and the concentration of wealth were sufficiently general or intense to create serious social conflict, even in older settled areas.

A second major question raised by these two hypotheses is the extent to which there were significant variations in the nature of social development, not only from colony to colony, but also from region to region within the colonies and even from one segment of society to another within a given community. From what we already know, it is clear that the differences were enormous, and the extent of these differences suggests that it is improbable that either or both of these hypotheses can subsume all of them.

Finally, the third, and for the present discussion much the most important and pressing, question posed by these hypotheses is the precise relationship between the many manifestations of social strain they have described and the American Revolution. Several scholars, expecially Wood and Lockridge,[32] have asserted the existence of direct causal links. But these assertions have been accompanied by what Clifford Geertz has called "a studied vagueness" that conceals the fact that as yet no one has succeeded either in establishing the existence of such links or in specifying exactly what they may have been and how they may have functioned in the overall causal pattern of the Revolution.[33] To a large extent, this failure accounts for the perpetuation of the widespread conviction among students of the Revolution that no such links were present or that, if they were, they could not have been very important.

This conviction has been reinforced by the obvious fact that the

[31] Greven, 103-258, presents a considerable amount of evidence that would seem to support this suggestion.

[32] Wood, "Rhetoric and Reality," 24-32; Lockridge, *A New England Town*, 184-85.

[33] Clifford Geertz, "Ideology as a Cultural System," in David E. Apter, ed., *Ideology and Discontent* (Glencoe, Ill., 1964), 53.

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American Revolution failed to generate a *societal* revolution. Note that I purposely use the term *societal revolution,* which connotes "a discontinuous process of structural innovation," rather than *social revolution,* which implies no more than some "quantitative increments" or qualitative changes within an existing structure.[34] Though the Revolution obviously helped to accelerate some long-standing tendencies within colonial society toward greater political and social inclusion and more individual autonomy and although incremental changes in patterns of social organization, institutional arrangements, and values may be observed in many sectors of American life during the Revolution,[35] there do not seem to be any sharp discontinuities that cannot be traced primarily to exigencies created by the separation from Britain. In other words we do not need to look for strain, frustration, and dysfunction within colonial society, but we must look to the political conflict between Britain and the colonies to find a plausible explanation for these discontinuities.

But the limited character of the internal social changes produced by the Revolution obviously cannot be taken to mean that these various components of strain had no causal relationship to the Revolution. That social context has an important bearing upon political events is a truism, and because these components were all present to one degree or another in colonial society at the time the Revolution occurred, it may be safely assumed that they had some effect upon the Revolution. The main question, then, is not whether a causal relationship existed but what kind of a relationship it may have been.

At least four possibilities immediately present themselves. First, from the evidence offered in support of the first hypothesis discussed above, we may surmise that the severity of social strain was so great—the anomie, disorientation, frustration, and friction so intensive and extensive—within colonial society as to make it exceptionally prone toward revolution by creating either a single social crisis of such magnitude that it cut across and through most segments and areas of colonial life or a complex multitude of inter-

[34] See the useful discussion by Edward A. Tiryakian in "A Model of Societal Change and Its Lead Indicators," in Samuel Z. Klausner, ed., *The Study of Total Societies* (New York, 1967), 73.

[35] See, especially, Main, *Social Structure,* and Wood, *Creation.*

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locking local, group, and personal crises that converged to produce severe disruption in many strategic sectors of colonial society. Something of this sort seems to be what Wood is implying.[36] Second, a corollary of this argument, suggested by Heimert and others, is that the Great Awakening and presumably the many other changes that contributed to the social ferment of the mid-eighteenth century prepared the colonists intellectually and emotionally for the rejection of British authority after 1763 by calling into question or otherwise undermining confidence in the authority of traditional religious, social, and political institutions and leaders.[37] On the other hand, the findings on which the second hypothesis rests can be marshaled in support of still a third argument which is a variant of the old formula of the Progressive historians: that declining opportunity and a rigidifying social structure created fierce resentments among the middle and lower classes against the elite that came surging to the surface of public life during the pre-Revolutionary years and these resentments merged with the protest against Britain to generate an internal revolution.[38] Still a fourth alternative is suggested by combining the two hypotheses: that, however upsetting it may have been to older members of the elite, the economic and demographic expansion and social ferment through the first five decades of the eighteenth century created rising economic and social expectations among the rest of society which were subsequently frustrated by a decline in opportunity beginning about midcentury. According to this formulation, which is merely an application of James C. Davies's general theory of revolution to the American Revolution,[39] the most critical element in turning American society toward revolution was not the erosion of social coherence or even the decline of opportunity but the frustrated expectations of a large segment of the population as a result of a general closing of society after a prolonged period of apparently becoming increasingly more open.

[36] "Rhetoric and Reality," 24-32.

[37] See also the interesting suggestions about the relationship between religious revivals and societal revolutions by Tiryakian, 94-95.

[38] Summaries of the arguments of the Progressive historians may be found in Greene, *Reappraisal*, 7-17, and "Changing Interpretations," 153-56.

[39] Davies, 1-19.

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Some or all of these theories may turn out to be appropriate vehicles for relating to the Revolution the various components of social strain in mid-eighteenth-century America. So far, we have been offered little more than a kind of superficial historicism that suggests that the behavior of the generation of the Revolution somehow emerged from or was conditioned by the social environment, a suggestion that is obviously "so general as to be truistic."[40] Before we will know which, if any, of these four theories are appropriate, we will have to be able to specify much more precisely than we can at present the ways in which those manifestations of strain impinged upon and affected the actual behavior of particular groups and individuals during the 1760s and 1770s; how, when, where, why, and to what extent the uncoordinated individual and group dissatisfactions that may have been produced by those strains merged into the collective opposition to Britain; and whether, in what sense, and by what sorts of psychological mechanisms those social strains actually did find "mitigation through revolution and republicanism," as Wood has contended.[41]

In the meantime, two different categories of existing studies provide some—but only some—help in this regard. By revealing, if in many cases only implicitly, the connection between political factionalism and particular sorts of social, economic, and religious strain, the rather extensive literature on political divisions within individual colonies between 1750 and 1776 and their relationship to the Revolution provides some general indications of what the answers to at least some of these questions may be for several colonies. In particular, this literature makes it clear that there were enormous variations from one colony to another and that virtually every colony had its own specific forms of social strain.[42] Recent studies of the many symbolic representations of internal social strains in Revolutionary rhetoric have confronted more directly the general question of the connection between such strains and the Revolution. They have shown how the colonists' fears about moral decline and the increase in self-oriented behavior in their

[40] Geertz, 53.

[41] Wood, "Rhetoric and Reality," 30. For a persuasive argument on the necessity of providing precise answers to such questions, see James Rule and Charles Tilly, "1830 and the Unnatural History of Revolution," *Journal of Social Issues* (forthcoming).

[42] See the discussion in Greene, "Changing Interpretations," 159-72.

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own societies merged with the opposition to Britain to turn the Revolution into a "norm-oriented movement" that looked forward to the regeneration of American society and the restoration of traditional social norms.[43] There can be no doubt that this projection of American corruption upon Britain helped make it easier to reject the monarchy and the British connection and to accept revolution and republicanism. To what extent it also relieved Americans of the guilt produced by their own moral failures and thereby actually helped to mitigate an important source of social strain within the colonies is not so clear. Indeed, by turning them toward republicanism with its even higher and less attainable standards, it may actually have exacerbated such strains.[44] Moreover, in the absence of any systematic attempt to assess the *relative* importance of these fears of moral decline in mobilizing widespread popular support for the Revolution, it is extremely difficult to know what causal weight they may have actually borne.[45]

IV

Whenever and however these crucial problems are resolved, it is doubtful that we will be able to assign to any or all of the components of social strain so far discussed a major causal role in the Revolution. To do so we would have to show that the extent of strain was infinitely greater and the degree to which they impinged themselves on individuals and groups was much more powerful than anyone has so far suggested or than our present knowledge seems to warrant.[46] It is extremely doubtful that we will be able to say even that any or all of those components were a necessary

[43] See, especially, Miller, 322-68; Morgan, 3-43; and Wood, *Creation*, 1-124. On "norm-oriented movements," see Smelser, 109-11, 270-312, and Tiryakian, 93-94.

[44] Wood's own evidence in *Creation of the American Republic* would seem to suggest as much.

[45] Heimert has attempted to show the existence of certain continuities between the intellectual and social divisions of the Great Awakening and those of the Revolution, but enough individual exceptions to his argument have been noted to raise serious questions about its general applicability. In particular, see Bernard Bailyn, "Religion and Revolution: Three Biographical Studies," *Perspectives in American History*, IV (1970), 85-172.

[46] If these strains had been a source of major discontent, we would expect to find a significant increase in the extent and intensity of such indices of social strain as crime, insanity, and both individual and col-

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cause of the Revolution, that is, that without them there would have been no Revolution. In all probability, we will be able to say only that these several components—for the most part, the regular concomitants of the normal processes of incremental change within colonial society—affected the Revolution in the same sense that they would have affected any other major political event that occurred in the context of which they were a part. Thus, they may be said to have "aggravated," perhaps in some cases even "intensely," "antagonism to the imperial system" and to have "fed into the revolutionary movement."[47] But they cannot be said either to have created the movement or to have been necessary for it to occur.

But this is not in any way to suggest that an understanding of the complex internal social, economic, political, and religious changes that were taking place within the colonies is not essential to a comprehension of many aspects of the Revolution or that it would be desirable either to relegate them to a residual role or to eliminate them from further discussions of the causes of the Revolution. On the contrary, existing knowledge suggests and future investigations will surely confirm that there was a direct relationship between the degree and special character of social malintegration and the particular configuration of the Revolution in each colony and that an understanding of the nature and intensity of internal strain within a given colony during the quarter-century prior to independence would enable us to predict what general shape the Revolution would assume in that state. If this suggestion is correct, then it is obvious that an analysis of the character and operation of various elements of social strain within the colonies is absolutely crucial to any comprehension of the many variations in the form and nature of the Revolution from one colony or one region to another.

For analytical and heuristic purposes, we might say, for instance, that only if every one of the components of strain so far identified and discussed as well as any that may subsequently be discovered impinged directly upon the Revolution in a given colony would we be likely to find the Revolution in its fullest and

lective violence in the colonies. No systematic study has been made of this subject, and it is obviously an area worth intensive investigation.

[47] Wood, "Rhetoric and Reality," 27.

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most extreme form. For simplicity, we can call this most extreme form of the Revolution *R*. It then follows that the absence of one or more of these components in another colony will result in something less than all of *R*. What we can finally say, then, about the causal importance of all of these strain-producing elements—perhaps all we can say—is that without any or all of them, we would not have all of *R*.

The larger point is that without the conflict between Britain and the colonies these components of social strain would not by themselves have produced a revolution. In any explanation of the causes of the Revolution, then, what one wants to know about such components is how they related to the Anglo-American conflict, how they may be used to illuminate the traditional and still central question of how and why the old structure of interests, institutions, and symbols—the sacred moral order—that had bound the colonies to Britain for the previous century broke down so rapidly during the third quarter of the eighteenth century. For that breakdown—as well as the frenzied rhetoric that accompanied it—can be explained largely by the anxieties deriving out of what the vast majority of both the colonial and British political nations took to be a series of fundamental violations of that sacred order by the other side, without invoking whatever frustrations or resentments may have arisen out of social strains within the colonies.

If these conclusions turn out to be correct, they raise serious doubts both about the argument—implicit in the theories of Chalmers Johnson, Neil J. Smelser, and other modern analysts of revolution—that "true revolutions," to quote Wood, must derive from "sources lying deep within the social structure"[48] and about the utility of such generalized themes of social change in explaining the origins of political revolutions. Certainly, the example of the American Revolution would seem to make it clear that social strain is a concept of limited value for explaining at least one variety of great modern political revolution. As James Rule and Charles Tilly suggest in a persuasive critique of these and other natural-history models of revolution, the central feature of any political revolution is "the seizure of power over a governmental apparatus by one group from another," and such a develop-

[48] *Ibid.*, 26.

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ment can usually be explained largely as a result of some conflict between two or more contenders within the polity over one or more issues of great moment. These issues may have social or economic, religious or cultural content, but they are invariably political because they are fought out in a political arena and involve questions of basic concern to the polity. What we need to know, then, to understand what caused a particular revolution is what specific conditions—short-term, intermediate, or long-term—led to (1) the appearance of rival contenders for authority and legitimacy; (2) the acceptance of the claims of the revolutionaries by a significant or strategic segment of the politically relevant population; and (3) the unwillingness or incapacity of the old regime to suppress its opponents. Usually, there is no special need to search for the existence of "anomie, strain, dysfunction or frustration" to explain these events. Wherever such evidences of disequilibration are present, they may well feed into the revolutionary situation; but, because they will usually be found to exist both before and after the revolution and at any number of intervening periods when no revolution occurred, they "fail to characterize situations that are distinctly revolutionary, and thus lack [much] explanatory power."[49] Not only can they not in themselves provide a sufficient explanation for many modern revolutions but in cases like the American Revolution they cannot even be said to have been necessary for them to occur.

The limited causal importance of the many evidences of social strain we have been discussing in this essay does not imply that we should abandon their study; but it does suggest that they should be approached from a different and considerably broader perspective. Whatever their relationship to the American Revolution, they were obviously manifestations of fundamental, long-term social changes taking place within the colonies. To the very great extent that those changes were in the direction of more rational and less traditional patterns of social, economic, and political relations, institutional structures, and values that put ever higher premiums upon individual autonomy, self-fulfillment, pursuit of self interest, and that thus emphasized economic returns, accumulation, achievement rather than ascription, functional spec-

[49] Rule and Tilly.

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ificity of economic and social roles, and universalistic criteria for membership in the polity and society, these changes were part of what E. A. Wrigley has described as a "revolution of modernization,"[50] a sweeping social revolution that had been in progress in Western Europe since the middle of the fifteenth century and, by the time of the American Revolution, had been underway in the colonies for at least a century.

This broader social revolution, which in many respects is far more crucial to an understanding of the first two centuries of American life and far more worthy of scholarly attention than the American Revolution, would have been completed with or without the American Revolution, albeit perhaps at a different rate of speed and in a somewhat different form,[51] and it would be unfortunate if this broader revolution continues to be confused with the American Revolution. For however interwoven it may have been with the American Revolution during the brief span of time between 1760 and 1790, there is always the danger that an excessive concentration upon discovering and specifying the links between the two will not only divert attention from and obscure the nature of the broader social revolution but will also contribute to prolonging the unfortunate American tendency to view social history as primarily interesting for what it can tell us about politics.[52] As E. J. Hobsbawm has recently warned, the value of studies of "major transformations of society" may well be "in inverse proportion to our concentration on the brief moment of conflict." It is a commonplace, of course, that political revolutions, by bringing into the open and dramatizing "crucial aspects of social structure" that would remain hidden or obscure in normal times, provide a mag-

[50] E. A. Wrigley, "Modernization and the Industrial Revolution in England," *Journal of Interdisciplinary History*, III (1972), 225-59. The definitions of *traditional* and *modern* implicit in my passages are derived from this article.

[51] Jackson Turner Main, "The American Revolution as a Social Incident," unpublished paper presented at the Symposium on the American Revolution, Williamsburg, March 10, 1971, p. 12, comes to a similar conclusion.

[52] Among several important recent works which attempt to break out of the prison of political history, see, especially, the general book-length essay by Rowland Berthoff, *An Unsettled People: Social Order and Disorder in American History* (New York, 1971), and the specialized study by Greven, *Four Generations*.

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nificent window into the societies in which they occur.[53] But the particular phenomena thereby observed will only appear in their fullest meaning when they are fitted into the wider context of the long-term tendencies of which they are a part. Only when such a context has been established will we finally be able to understand the precise relationship between America's late eighteenth-century political revolution and its peculiar variant of the wider social revolution of modernization that, by the end of the eighteenth century, was sweeping through much of the Western world.

[53] E. J. Hobsbawm, "From Social History to the History of Society," *Daedalus*, C (1971), 39-40.

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Theories of Revolution in Contemporary Historiography[1]

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Revolution has been the subject of investigation by philosophers and historians since almost the beginning of the Western intellectual tradition. Plato and Aristotle were the first to deal with revolution as a theoretical problem and Thucydides the first to give it reflective historical treatment.[2] No one who reads it is likely to forget Thucydides' penetrating account of revolutionary psychology and the effects of class war upon the Greek states. It is highly critical of revolution, yet is, on the whole, the fruit of profound insight and detached observation. From the time of Thucydides to the eighteenth century, a succession of historians, most of them men of affairs who wrote on history and politics as an avocation or as the occupation of exile or retirement, have thrown fitful light upon revolution and civil war. To this company belong Sallust, Machiavelli, Commines, Davila, de Retz, Harrington, Clarendon, and Montesquieu, together with some other well or

[1] Other discussions of this subject which will be found useful are Lawrence Stone, "Theories of Revolution," *World Politics*, XVIII (1966); Henry Bienen, *Violence and Social Change* (Chicago, 1968), especially chap. 3; and Charles Tilly, "Revolutions and Collective Violence," in Fred I. Greenstein and Nelson Polsby, eds., *Handbook of Political Science* (forthcoming). I am obliged to Professor Tilly for allowing me to see a copy of his paper.

[2] Plato, *Republic*, Bk. VIII; Aristotle, *Politics*, Bk. V; Thucydides, *The Peloponnesian War*, III, 81-84.

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little known authors. It was not until the nineteenth and twentieth centuries, however, that revolution became one of the supreme and central preoccupations of historiography. Since the French Revolution, the volume of work devoted to the various revolutions of the Western countries has increased enormously, while, more recently, interest has extended farther to include the revolutions of Asia and Africa as well.

The prominence that revolution has assumed in historical study is generally thought to be mainly due to the influence of revolutions themselves in shaping the modern world. This influence became powerful with the French Revolution, which both by its actual character and by the mythologies it inspired, opened an era of profound change in human affairs. In 1850, Alexis de Tocqueville, commented on the extent of the change:

> For sixty years we have been deceiving ourselves by imagining that we saw the end of the Revolution. It was supposed to be finished on the 18th Brumaire, and again in 1814; I myself thought in 1830, that it might be over when I saw that democracy, having in its march passed over and destroyed every other privilege, had stopped before the ancient and necessary privilege of property. I thought that, like the ocean, it had at last found its shore. I was wrong. It is now evident that the tide is rising, and that the sea is still enlarging its bed; that not only have we not seen the end of the stupendous revolution which began before our day, but that the infant just born will scarcely see it. Society is not in process of modification, but of transformation.[3]

These words were written more than a century ago, but who would say that they are no longer applicable? Although the revolution of which Tocqueville spoke has constantly assumed fresh forms, although it has metamorphosed into communist, fascist, racial, and colonial revolutions, the process that he recognized has nevertheless retained all of its dynamism.

During the last few years the word "revolution" has also been heard a great deal in the United States. The American postwar consensus, which some had imagined to be permanent, broke down in the 1960s. The so called "end of ideology," happily celebrated in the 1950s by many academic intellectuals, turned out to be an illusion. Various revolutionaries, both real and merely self-styled, came upon the scene, and in the angry babble of argument

[3] Alexis de Tocqueville, *Memoirs, Letters, and Remains of Alexis de Tocqueville* (London, 1861), I, 423.

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that followed, the possibility of a revolution in the United States also began to be discussed for the first time since the 1930s.

A number of revolutions have recently proclaimed themselves in America: the black revolution, the student revolution, the women's revolution, and that miraculous occurrence prophesied by Professor Reich and known as Consciousness III or the youth revolution.[4] There have been manifestations of a similar kind in Europe and Japan. The belief in the necessity of a redemptive revolution that will cleanse the advanced industrial world of its accumulated evils has taken root in various quarters. At the same time, significant developments have occurred also in revolutionary strategy. The teachings of Mao Tse-tung and General Giap have gained disciples in the West. Régis Debray's important book about Latin America, *Revolution In the Revolution?*, contains a rationale for the ideas of Castro and Guevara, which are intended to shake up the Communist parties, launch guerilla insurgencies, and thereby create revolutionary situations capable of bringing Communists to power.[5]

Admittedly, much of what passes for revolution nowadays is only light-minded talk and frivolous posturing. Some of it, however, deserves to be taken seriously. Moreover, the increasing frequency of violent episodes of protest and repression has aroused questions about the stability of American institutions and their adaptiveness to peaceful change. Connected with this questioning has been a sudden outburst of writings and studies dealing with the causes of violence in America. Partly for the same reasons and partly because of the revolutionary challenges operating in the underdeveloped world, there has also been a marked revival of intellectual interest in the problem of revolution. Thus, revolutions, which the revolutionist Leon Trotsky called "the mad inspirations of history," have acquired a fresh pertinence.[6] It is therefore worth seeing the place of revolution at present in the intellectual landscape of history and the social sciences.

I

To proceed, we must raise the important and controversial ques-

[4] Charles Reich, *The Greening of America* (New York, 1970).
[5] Régis Debray, *Revolution in the Revolution?* (New York, 1967).
[6] Leon Trotsky, *My Life* (New York, 1930), 334.

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tion of the definition of *revolution*, the source of considerable disagreement. A glance at the history of the word itself may provide us with some help in this matter.[7] It still seems not to be generally realized that before the seventeenth century, *revolution* possessed quite different connotations from those associated with it today. The word was then scarcely used at all in a political sense, but referred mainly to the circular motion of the planets. During the seventeenth century it gradually acquired a political meaning also, but retained the idea of circularity. Thus, even as a political occurrence, *revolution* was understood merely as a synonym for the cycle of change in states, a cycle of turbulent ups and downs. The first rebellion of modern times widely called a revolution by its contemporaries was the English Revolution of 1688. Nevertheless, these same contemporaries, as well as many later writers, did their best to depict the revolution against James II as conforming to the circular model—as a "restoration," a "return" to a system of legality that had been violated by a tyrannical king. Accordingly, what is noticeably absent from the meaning of *revolution*, even at the end of the seventeenth century, is any connection with innovation and the inauguration of a new order. For the purposes of this discussion, let it suffice to say that after 1789, *revolution* vastly enlarged its reference. The upheaval in France infused the term with a new potency and made it a call to action, a shibboleth, a mystique. Marxism in due course reinforced this significance. It became linked with ideas of progress and the conscious shaping of history. It began to signify the willed, deliberate effort to create a new society, a new humanity, and a new world. Nineteenth-century thinkers, whether conservative, liberal, or socialist, were largely in accord in viewing revolution as a phenomenon of epochal change and innovation.

Thus, by an indefinite process of extension, *revolution* has come to contain a vague, assorted medley of meanings and to be applied to developments of the most varied kinds. Historians, in consequence, have given us an almost endless number of different revolutions, such as, for instance, the industrial, commercial, scientif-

[7] Cf. Perez Zagorin, *The Court and the Country* (New York, 1970), chap. 1, and the literature cited there for a fuller discussion of this subject.

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ic, and Protestant revolutions; the intellectual, educational, and military revolutions; and the urban, neolithic, population, second industrial, and sexual revolutions. This loose and confusing use of the term merely illustrates how closely the idea of revolution has become equated with change. It would seem that all macroprocesses of change, whatever they are, must be described as revolutions. Nevertheless, few of these revolutions have anything in common with one another or with the concept of revolution itself defined in any precise sense. Hence, although I have little hope that they will do so, historians would add greatly to clarity if they ceased the proliferation of revolutions and reserved the term for a single, reasonably well marked class of events.

What should its definition be? A recent suggestion, adopted by some political scientists, is to substitute for *revolution* the term *internal war*, defined as "any resort to violence within a political order to change its constitution, rulers, or policies."[8] While the use of *internal war* has provoked some illuminating discussion, it is hard to see the superior merit claimed for it. It also has the defect that certain kinds of revolutions, such as some colonial revolts or military coups d'état, would not normally be called internal wars. In addition, internal war is analogous to civil war and, therefore, rather than being *sui generis*, is best seen as one possible phase or stage in the development of a number of different types of revolution. Hence it seems preferable to retain the well established word *revolution* in a clearly delimited context describing change which is characterized by violence as a means and a specifiable range of goals as ends.

A well known line of thought proposes to restrict the term *revolution* solely to movements with goals involving far-reaching changes in social structure, class domination, institutions, and ideology. This view has been characteristic of the Marxist approach although it is held by non-Marxists as well. In effect, it accepts only the greatest revolutions as revolutions. A recent version of this opinion is advanced by S. P. Huntington: "A revolution is a rapid, fundamental and violent domestic change in the dominant

[8] Harry Eckstein, "On the Etiology of Internal Wars," *History and Theory*, IV (1965), 133; cf. also, H. Eckstein, ed., *Internal War: Problems and Approaches* (New York, 1964).

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values and myths of society, in its political institutions, social structure, leadership, government activity, and policies."[9] This definition, however, is arbitrary in what it excludes and is, therefore, of little use to the historian. It has no room for most peasant revolts, urban insurrections, and provincial or national separatist rebellions, to mention only a few. To adopt it would only result in a pointless narrowing of the field for both the comparative and theoretical study of revolution.

Perhaps it is impossible to establish a completely satisfactory definition of the term, so complex are the phenomena and variables to be included. Of the definitions available, the most useful appears to be the one adopted by Chalmers Johnson in his two books.[10] It conceives a revolution as violence directed toward one or more of the following goals: a change of government (personnel and leadership), of regime (form of government and distribution of political power), or of society (social structure, system of property control and class domination, dominant values, and the like). It would be desirable to add to this list a change of governmental policy as well. With that addition, Johnson's formulation seems reasonably exact and appropriate to all or most varieties of revolution, whatever their differences in aim, scale, or social character.

Accepting this working definition, we are in a position to ask in what ways the problem of revolution has been attacked. Three possible lines of inquiry exist, which even though they may overlap, can nonetheless be clearly distinguished from one another. The first, which is also the best worked and most familiar, is historical in the strictest sense. It is directed to the investigation of a specific individual revolution like the French or the English Revolution or the Fronde. Historians taking this approach have achieved outstanding successes, with the result that almost everything we know about revolutions comes from their detailed researches and general accounts. The second kind of inquiry is comparative. Its procedure is to deal with revolutions as a class, selecting from this class two or more instances in order to investigate

[9] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, 1968), 264.

[10] Chalmers Johnson, *Revolution and the Social System* (Stanford, 1964) and C. Johnson, *Revolutionary Change* (Boston, 1966).

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any significant relationships between them. In this field, like that of comparative history in general, historians and social scientists have accomplished something, but not a great deal so far. Finally, the third kind of inquiry is theoretical. Its purpose is to establish a theory of revolution capable of explaining its causes, processes, and effects as a type of change. Various hypotheses have been advanced within the social sciences, but despite the suggestiveness of some of these, nothing has appeared that qualifies as a general theory of revolution. Furthermore, among theorists there has been little progressive accumulation of ideas. The general theory of revolution remains subject to confusion, doubt, and disagreement. Even elementary questions of definition, terminology, and delimitation of the field to be explained are still not settled.

II

Before looking at recent work on the theory of revolution, it is useful to mention some earlier attempts to deal with the subject. Previous efforts to establish a theory of revolution have concentrated primarily on causation. Other problems, such as the classification of revolutions, the investigation of the dynamic processes involved in revolutions, and the study of the long-range consequences of revolutions, have been neglected by comparison. To account for revolution, many factors and conditions have been adduced. In European history, the French Revolution affords the classic case in which causal primacy has been imputed to factors of the most diverse kinds—social, economic, intellectual, psychological, and so on. Tocqueville thought that revolution was likeliest to happen when oppression was being lightened. Marx, on the other hand, connected the development of revolution with intensified oppression or exploitation. In 1944, the American historian Louis Gottschalk published an article proposing five general causes of revolutions, such as provocation by regimes, solidified public opinion, hopefulness of change.[11] A list of this kind, however, is too nebulous to contribute much to the development of a theory of revolutionary causation.

Between the two world wars, prior to the recent renewal of in-

[11] Louis Gottschalk, "The Causes of Revolution," *American Journal of Sociology*, L (1944), 1-8.

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terest, some writings of considerable merit dealing with the theory of revolution appeared in the United States. Among these were the works of two sociologists, Pitirim Sorokin and Lyford P. Edwards, and a political scientist, George S. Pettee.[12] The best known of earlier writings, however, is historian Crane Brinton's *The Anatomy of Revolution,* still probably the most influential as well as the most widely read book on revolution to have been written in this country.[13] Prior to the appearance of his book, in a monograph on the Jacobins, Brinton tested several hypotheses about the role of psychological and economic factors in attracting people to the revolutionary cause. He decided from an analysis of their membership that the Jacobins were neither misfits nor failures and that they included a large cross section of French society. His skeptical conclusion was that the Jacobins were actuated not by material or any other interests, but by a quasi-religious faith.[14] In *The Anatomy of Revolution,* Brinton attempted to discover certain uniformities by systematically comparing the English, American, French, and Russian Revolutions. In selecting these four, he followed most scholarship, which has tended to concentrate on instances drawn from the small class of so called great revolutions. Limiting the comparison to this category, however, affords a dubious basis for establishing uniformities. Even among the great revolutions, moreover, a number, like the Dutch Rebellion or the Mexican and Turkish Revolutions, for instance, are hardly ever considered. For Brinton some of the uniformities in the inception of revolution are an economically advancing society, an inefficient, financially hard-pressed government, class antagonisms, desertion of the existing order by the intellectuals, and loss of self-confidence by the ruling elite. He also perceives additional uniformities in the successive stages of revolution: the control of moderates gives way to the rule of extremists, which, in turn, gives way to a reign of terror followed by a Thermidorean reaction. Further uniformities appear in the effects of revolution, including great

[12] Pitirim Sorokin, *The Sociology of Revolution* (Philadelphia, 1925); Lyford P. Edwards, *The Natural History of Revolution* (Chicago, 1927); George S. Pettee, *The Process of Revolution* (New York, 1938).

[13] Crane Brinton, *The Anatomy of Revolution* (New York, 1938).

[14] Crane Brinton, *The Jacobins: An Essay in the New History* (New York, 1930).

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property transfers, the replacement of one ruling class by another, and the achievement of more efficient, centralized government.

It is doubtful whether anyone would accept these as uniformities since so many exceptions to them can be found. The English Revolution, for instance, was not caused by class antagonism, was not preceded by the desertion of the intellectuals, and did not produce a reign of terror. Similarly, a political scientist has written of the American Revolution that "we find neither victory of the extremists, nor the terror, nor the Thermidor, nor yet the 'tyrant' dictator who reestablished order. . . ."[15] Furthermore, neither in England nor in America did one ruling class replace another. In my opinion, Brinton's generalizations result from an uncritical tendency to treat the French Revolution as a model, a predisposition he shared with many others. Hence they apply much better to the French than to the other revolutions. Although brilliantly suggestive, *The Anatomy of Revolution* seems to me to offer in the main a number of partial resemblances rather than uniformities in any true sense.

Another early attempt to deal with the theory of revolution derives from Marxism, which has remained extremely influential in the historical treatment of revolution. It has dominated the historiography of the French Revolution to such an extent that its interpretation of the Revolution has become virtually canonical.[16] Marxist theories have also been frequently advanced to explain the English Revolution, and they figure as well in current controversies about the so called "general crisis of the seventeenth century."[17] Marxism has been, as well, one of the main influences upon the historical conceptualization of the nineteenth-century revolutions in France and other European countries. The much discussed question of the *rivoluzione mancata*, or failed revolution of

[15] Carl J. Friedrich, *Revolution: Nomos*, VIII (New York, 1966), 6.

[16] Cf. Alfred Cobban, *The Myth of the French Revolution* (London, 1955) and A. Cobban, *The Social Interpretation of the French Revolution* (Cambridge, 1964).

[17] From the large and continually growing literature on this subject, I need cite here only the papers contained in Trevor H. Aston, ed., *Crisis in Europe, 1560-1660* (New York, 1965); Lawrence Stone, ed., *Social Change and Revolution in England, 1540-1640* (London, 1965); and Robert Forster and Jack P. Greene, eds., *Preconditions of Revolution in Early Modern Europe* (Baltimore, 1970).

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the Italian Risorgimento has its origin in Marxist conceptions of the historical process.[18] And although it contains no statement of a formal theory of revolution, Marxist ideas have provided a considerable part of the explanatory framework of E. H. Carr's great *History of Soviet Russia*.[19]

Marxism has been a source of insight to historians of widely different views. In speaking of the Marxist theory of revolution, however, we are referring to Marxism in its integral character, not to Marxist ideas diluted in eclectic mixtures nor to those of Marx's ideas that are accepted by nearly all students of society. The outstanding merit of the Marxist model is its systematic integration of the factors of the economy, the social structure, the state, and ideology in order to explain revolution. Its defects include the attribution of ultimate causal determination, largely or wholly, to economic factors; its mistaken assumptions that economic class is always the dominant collectivity in a social structure and that class conflict is the sole source of revolutionary change; finally, its grossly simplified notion of an evolutionary succession of societies, with primitive communism giving way, in turn, to slavery, feudalism, capitalism, and so on, to which there corrresponds an equally simplified classification of revolutions.

Historically, Marxist scholarship has been most interested in bourgeois revolution and the transition from feudalism to capitalism. Its conceptual and terminological confusions on the subject are notorious. Feudalism has come to include anything from the eleventh to the nineteenth centuries and to designate conditions as far apart as a full-blown seigneurial regime, absolute monarchy, and an order in which landed aristocracies merely enjoy superior social prestige and authority. The bourgeoisie and bourgeois revolution are almost equally elastic categories. When Marxist historians speak of seventeenth- and eighteenth-century France as a feudal society or describe the imperialism of Hapsburg Spain as "the highest stage of feudalism," the result can only be confusion.[20] Admittedly, the analysis of class and of social structures

[18] Cf. A. W. Salomone, "The Risorgimento and the Political Myth of 'The Revolution that Failed,'" in A. W. Salomone, ed., *Italy from the Risorgimento to Fascism* (New York, 1970).

[19] E. H. Carr, *History of Soviet Russia* (London, 1950-71).

[20] Cf. Cobban, *Social Interpretation*, for some illustrations. The feudal conception of seventeenth-century France is advanced in B. Porshnev, *Les*

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presents complicated problems and is a difficult undertaking. Marxist concepts used for this purpose, however, have not been rigorously or even clearly developed.

With the progress of research, the inadequacies of the Marxist theory of revolution have become increasingly evident. Marxist explanations of the English Revolution have fallen into abeyance because of the damaging criticisms to which they have been subjected. In the case of the French Revolution, attempts to preserve the Marxist scheme resemble the addition of epicycles to the Ptolemaic system in order to save the phenomena. The accumulation of knowledge about the complexities of French society has made it clear that the Marxist analysis is far too abstract and too crude. Marxists trace the revolution to a conflict between the aristocracy and the bourgeoisie, but concrete evidence for so reductionist a dichotomy is lacking. A leading French historian, Pierre Goubert, has recently pointed out in a penetrating study of the *ancien régime* the absurdity of any longer trying to explain the revolution as "the triumph of an unidentifiable capitalist bourgeoisie over an unidentifiable feudal aristocracy."[21] Severe criticisms of the Marxist class analysis have also come from Professors Mousnier and Cobban.[22] Even E. H. Carr, who is more sympathetic to the Marxist view, has pointed out that the French Revolution cannot be described as a bourgeois revolution in the sense that it was begun or led by an identifiable class.[23] When Marxists resort to explanations of a still more general character, the difficulties become even worse. For example, Professor Soboul, who has done important research on the popular movement in revolutionary Paris, locates the ultimate cause of the French Revolution in the contradiction between the productive forces and the relations of production.[24]

soulèvements populaires en France de 1623 à 1648 (Paris, 1963). Cf. also, D. Parker, "The Social Foundation of French Absolutism, 1610-1630," *Past and Present*, LIII (1971), 67-89. For the "feudalism" of seventeenth-century Spain, cf. P. Vilar, "Le temps du 'Quichotte,'" *Europe*, XXIV (1956), translated in *New Left Review*, LXVIII (1971), 59-71.

[21] Pierre Goubert, *L'Ancien Regime* (2nd ed.; Paris, 1969), 257.

[22] Cf. the papers collected in Roland Mousnier, *La plume, la faucille, et le marteau* (Paris, 1970); Cobban, *Social Interpretation*.

[23] E. H. Carr, *The October Revolution* (New York, 1969), 16-17.

[24] Albert Soboul, *The Parisian Sans Culottes and the French Revolution, 1793-94* (Oxford, 1964). The doctrinarism which overlays the acute perception and rich concreteness of Soboul's work has been noted by

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This proposition is so murky and abstract that it eludes any possibility of verification. Perhaps it should be received as a profession of faith rather than as an historical statement.

If one asks about Marxism the question that Croce in a famous essay asked about the philosophy of Hegel—what is living and what is dead in the Marxist theory of revolution?—the answer must be that, in large part, it has been superseded. Its inadequacies might have been thought to have been conclusively revealed when Marx's own prediction that revolutions would occur in the most advanced countries of the West did not come true, and, instead, socialism triumphed in the backward peasant societies of Russia and China. With the growth of research on many revolutions, so much of the Marxist model has been disproved or shown to be untenable because of its oversimplifications that it has become in many ways an obstacle to further understanding. What remains of value in this model are some of the problems it poses for investigation and its emphasis upon the importance of economic and social-structural factors in the analysis of change and revolution. These will undoubtedly set challenges to historians, sociologists, and political scientists for a long time to come.

III

Since about 1960 there has been a noticeable revival of interest among social scientists other than historians in revolution as a theoretical problem. This interest has manifested itself in a number of writings by political scientists and sociologists dealing with revolution in various theoretical contexts. At the same time, the present position of historiography with respect to the investigation of revolution also presents certain features that call for mention. Although I have been referring to history as a social science, many historians would either reject or wish to qualify that description. In addition to the continuous fragmentation of historiography into innumerable specialties, there has been an increasing separation between the humanistic and social-science approaches

another historian of the popular movement in the French Revolution; cf. Richard Cobb, *The Police and the People: French Popular Protest, 1789-1820* (Oxford, 1970), 120-21, 186.

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to historical inquiry. This seems to have reached the point where it may be asked if history remains any longer a single discipline. The same question has been raised in a recent volume edited by David Landes and Charles Tilly which is part of the survey of the behavioral and social sciences conducted under the auspices of the Social Science Research Council and the National Academy of Sciences.[25] There the humanistic and social-science modes are contrasted as quite different types of inquiry. Probably most historians would place themselves somewhere between the two extremes, but nearer the humanistic than the social-science approach. In view of the distinction, however, it is significant that the same survey should indicate that revolution is one of the focal subjects of social-science history. This is certainly true of much of the more interesting work being done. Social-science history is not characterized primarily by the use of quantitative techniques; it is mainly characterized by a certain manner of formulating problems, by its effort at empirical rigor, by its use of theories, models, and ideal types, by its interest in comparative and interdisciplinary studies, and by the orientation of its research toward the understanding of whole societies. In history, as in other fields, most of the current efforts to gain a clearer understanding of revolution proceed from or are influenced by this approach.

An example of current scholarship is the research on what might be called revolutionary populations. The actors in revolutions belong either to elite groups or to the masses. Systematic analysis of the characteristics of each group using comparative data on the individual members can lead to enlightening results. Research of this kind can center on anything from the membership of formal institutions and ruling bodies to the participants in protest incidents as shortlived as a riot. From such work it is possible to gain a much fuller and more precise knowledge than we have ever had before of recruitment, of opposition to revolution, of leadership, of popular activism, and even of individual mentalities. The collective history of the Parisian *sans culottes* by Albert Soboul is an outstanding example. He reconstructed as far as possible in all its lineaments the mass movement in Paris during the

[25] David Landes and Charles Tilly, eds., *History as Social Science* (Englewood Cliffs, N. J., 1971).

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Jacobin ascendancy and the Terror by close investigation of the people who composed it.[26] George Rudé has performed a similar task in his dissection of the crowds and crowd behavior in the French Revolution and other social disturbances.[27] Another instance of a different kind is the detailed elite studies, like the analysis of the Soviet Politburo and of Nazi officialdom, done by political scientists.[28] Still another example is the investigation of the membership of the Long Parliament by Douglas Brunton and D. H. Pennington, from which it appears that the Royalist and Parliamentarian members as a body were socially and economically more or less indistinguishable from one another.[29] This research is not only valuable for the understanding of particular revolutions, but is also relevant to the theory of revolution because it can provide data for models and for the testing of explanations derived from general theories.

Another kind of work is the comparative history of revolution, of which there are various recent examples. A prominent one is R. R. Palmer's *The Age of the Democratic Revolution*, which gives a valuable account of political developments between 1760 and 1800 and also conceives of the French Revolution as the central event in an interrelated upsurge of revolutions extending over America and Western Europe.[30] Most of the discussion provoked by Palmer's book has centered on the question of whether there really was an eighteenth-century revolution of the West. Although Palmer has put much material together with great skill, his work contains little on the wider problem of revolution itself. It touches passingly on the causes of revolution, on revolutionary psychology, on the basis of political allegiances, and similar problems, but these reflections are too vague to be very illuminating. Palmer has also written an essay on generalizations about revolution which is equally cursory and vague in regard to theoretical

[26] Soboul. Similarly valuable is the work of Cobb, despite his "total rejection of sociology and quantification," p. xv.

[27] George Rudé, *The Crowd in the French Revolution* (Oxford, 1959), and G. Rudé, *The Crowd in History* (New York, 1964).

[28] Harold Lasswell and Daniel Lerner, eds., *World Revolutionary Elites* (Cambridge, Mass., 1965).

[29] Douglas Brunton and D. H. Pennington, *Members of the Long Parliament* (London, 1954).

[30] R. R. Palmer, *The Age of the Democratic Revolution* (Princeton, 1959-64).

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issues.[31]

Work of more direct theoretical interest has appeared in recent writing dealing with sixteenth- and seventeenth-century revolutions and "the crisis of the seventeenth century." The differences arising from these studies, which address themselves to basic questions concerning the societies, governments, and revolutions of early modern Europe, have led to some of the liveliest controversies in contemporary historiography. Contributions to the subject have come from H. R. Trevor-Roper, Eric Hobsbawm, J. H. Elliott, Lawrence Stone, J. H. Hexter, Perez Zagorin, Roland Mousnier, and others, who have provided searching discussions of the determinants of social structure and political differences in connection with the revolutions of the period.[32] Models for these revolutions have also been suggested, such as the court-country opposition or the conflict between the warlike, top-heavy absolutist state and society. One of the most active scholars in all these problems is Professor Mousnier of the Sorbonne. He has compiled an extremely detailed questionnaire to guide research on rebellions in France between the end of the Middle Ages and the French Revolution. In a number of historical and comparative studies, he has also stressed the importance of aristocratic leadership for the type of revolt that occurred in the societies of the *ancien régime*.[33]

Millenarianism, another large topic, has been the subject of comparative studies, in which the central problem is its relationship to modern revolutionary movements. On this there has been a convergence of research by historians, anthropologists, and sociologists.[34] While by no means uniform in character, millenarian movements in Europe have usually displayed two features: first,

[31] R. R. Palmer, "Generalizations about Revolution: A Case Study," in Louis Gottschalk, ed., *Generalization in the Writing of History* (Chicago, 1963).

[32] Cf. the writings cited above in n. 17 and the literature referred to in these works.

[33] Roland Mousnier, *Recherches sur les soulèvements populaires en France de 1485 à 1787: Questionnaire* (Paris, Cêntre de Recherches sur la Civilisation de l'Europe Moderne, n.d.); R. Mousnier, *Fureurs paysannes: Les paysannes dans les révoltes du xvii^e siècle (France, Russe, Chine)* (Paris, 1967); R. Mousnier, *La plume.*

[34] Cf. Sylvia Thrupp, ed., *Millennial Dreams in Action*, in *Comparative Studies in Society and History*, Supplement II (The Hague, 1962), and Yonina Talmon, "Millenarism," in the *International Encyclopedia of the Social Sciences* (New York, 1968), X, and Y. Talmon, "Pursuit of the

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the complete rejection of the existing world order and the expectation of a future new world purged of the evils of the old; second, an ideology of total transformation derived from Jewish-Christian apocalyptic beliefs. A further feature frequently associated with millenarianism is the presence of a charismatic leader or a prophet. Many writers have noted the typological resemblance between such religious movements and secular revolutionary movements, and some have also argued the historical dependence or interconnection of the two. One approach to the problem is exemplified in Hobsbawm's *Primitive Rebels,* which treats millenarianism as a prepolitical phenomenon arising in traditional societies and among the backward strata of more advanced societies. Millenarian movements in this view are a revolutionary expression of deprived and oppressed groups who have not yet attained political consciousness and who lack the means of political organization.[35] An alternative approach is taken by Norman Cohn in *The Pursuit of the Millennium,* who stresses not only socioeconomic factors but also the psychological elements in millenarianism which tend toward producing outbreaks of collective hysteria and irrationality. In the case of rebellions such as the German Peasant War of 1525, Cohn is inclined to see millenarianism as a factor injected by a small number of fanatical enthusiasts into a much larger popular movement with realistic aims. He also points out the importance of intellectuals in framing the ideology of millenarian revolts.[36]

Hobsbawm's analysis regards millenarianism both as the expression in mystical terms of social and economic interests and as the precursor of realistic political struggle. It also assumes an evolutionary scheme, distinguishing archaic from modern forms of rebellion, the highest stage of which is revolutionary Marxism. His assumption, however, is questionable. It seems more likely that rather than being a phenomenon peculiar to primitive social forms, futurist fantasies of transformation and perfection are a possibility in any society and are capable of combining with highly realistic beliefs and a sophisticated political organization. They

Millennium: The Relation Between Religious and Social Change," in Barry McLaughlin, ed., *Studies in Social Movements* (New York, 1969).

[35] Eric Hobsbawm, *Primitive Rebels* (Manchester, 1959).

[36] Norman Cohn, *The Pursuit of the Millennium* (Fairlawn, N. J., 1957), and N. Cohn, "Medieval Millenarism," in Thrupp, 37-39.

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are an important element, for instance, both in Leninist and Maoist thought and in the utopian expectations of contemporary youth revolutionaries. Millenarianism can therefore be considered not only as defining one particular species of rebellion, but also as a possible component in some secular types of revolution as well.

Perhaps the most noteworthy recent comparative study of revolution is Barrington Moore's *Social Origins of Dictatorship and Democracy*.[37] Moore's book exemplifies the current intellectual trend in the study of revolution by being the work of a sociologist concentrating entirely on a set of historical problems. What it seeks to explain is the process of transition from traditional agrarian to modern industrial society as experienced in a number of countries in the East and West; England, France, the United States, Japan, China, and India are examined, with frequent reference made also to Germany and Russia. The balance between the analysis of historical events and the drawing of theoretical generalizations is admirably maintained. Whatever criticisms historians may make, they are not likely to accuse it of the jejune and mechanical treatment often apparent in the writings of nonhistorian social scientists when they use historical materials.

Moore identifies three basic patterns in the modernizing process. The first leads via bourgeois revolution to capitalism and democracy of the Western type. Its instances include England, France, and also the United States whose civil war Moore explains as a revolution caused by the political incompatibility between slavery and a democratic capitalist order. The second, in which the bourgeois revolution fails, leads to capitalism with much weaker democratic features. This was the pattern for Germany and Japan which resulted in fascism. The third leads through great peasant revolutions to communist regimes that force the society into modernization. The examples are Russia and China. To these three basic patterns, Moore adds a fourth, exemplified by the case of India, in which, despite the achievement of parliamentary democracy, the impulse to modernization is weak because there has been neither a bourgeois nor a peasant revolution.

One variable is critical in the patterns described by Moore—namely, the way in which the traditional agrarian sector in each

[37] Barrington Moore, *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966).

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case has been subjected to capitalist relations and modernization, which, he believes, was, in turn, determined by the respective roles of peasants, landed elites, urban bourgeoisies, and the state. Where the process has been carried out by the bourgeoisie and part of the peasants, as in France, or by capitalistic landlords together with the bourgeoisie, as in England, the result is democratic capitalist regimes. Where it has been accomplished by landlords and the state bureaucracy without the presence of a strong bourgeoisie, as in Germany and Japan, the result has been authoritarian and fascist regimes. Finally, where landlords and state bureaucracies inhibited the process and the bourgeoisie was too small and weak to effect it, as in China and Russia, the impetus has come from peasant masses mobilized by communist parties, which then made the peasants their first victims.

Moore does not propose a general theory of revolution; rather he presents some generalizations about a certain range of experience in which revolution occupies the central place. The occurrence or failure of revolution, as well as its specific character, are viewed as crucial for the ensuing type of development. Revolution is thus placed within the historical process as a decisive point of conflict having significant systemic consequences. Moore dismisses the idea that revolution is necessary to remove the obstacles to modernization. He points out, however, that modernization achieved under conservative auspices retains many old structures and hence leads to quite different results from those produced by a great revolution. He accepts the conception of a bourgeois revolution, though he does not necessarily mean the seizure of power by a bourgeoisie. Revolutions, Moore holds, should be compared and classified with reference to their broad institutional consequences. Consequently, because of the kind of political and social order that resulted from them, he links the English and French Revolutions with the American Civil War as stages in the development of the bourgeois-democratic revolution.

There are a number of serious weaknesses in Moore's work. For one thing, in laying such heavy stress on economic relations, he underestimates or disregards the role of values and even of politics in social change. Thus in the case of Germany, conservative modernization may be a necessary cause of fascism, but it can hardly be a sufficient cause since it does not include other political cir-

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cumstances and value orientations that were fundamental to the outcome. In the case of England and France, to call both products of a bourgeois revolution does nothing to explain why the subsequent French political order, in contrast to the English, was so unstable and divided against itself. Moreover, German fascism, like Italian, was itself a revolution with broad systemic features. Moore's model, however, cannot include fascism as a type of revolution. Strong reservations also arise in connection with Moore's analysis of the American Civil War. Since he agrees with the conclusion of recent scholarship that Northern capitalism and Southern plantation slavery were not antagonistic, but existed in a profitable relation with one another as economic systems, it is difficult to see what ground there can be for explaining the Civil War as a bourgeois-democratic, capitalist revolution of the same type as the English and French.

Moore's work, nonetheless, must be regarded as a significant synthesis in the comparative treatment of revolution. It is also notable for its avoidance of excessive abstraction and its firm sense of historical reality. An approach of this kind, linking the comparative and theoretical dimensions of inquiry with the historical, is one of the most likely to lead to a better understanding of the nature and effects of revolution in large-scale processes of social change.

In addition to comparative studies, social scientists have also tried to develop some general theories and explanations of revolution. For instance, the question of how revolution is related to poverty and to economic and social oppression has always been puzzling. Although Marx explained revolutions as the effect of increasing misery and exploitation, Tocqueville, Brinton, and others, while not denying, of course, the importance of poverty, have argued that some of the greatest revolutions occurred in societies where the level of life was improving. Tocqueville is most explicit on this point, declaring that an unprecedented advance in the prosperity of the nation took place before the French Revolution and that this promoted a spirit of unrest and discontent which was highest in those parts of France which had experienced most improvement.[38] Today most students hold that revolution is least

[38] Cf. Alexis de Tocqueville, *The Old Regime and the Revolution*, trans. S. Gilbert (New York, 1955), pt. 3, chap. 4. For Marx the intensification

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likely among populations experiencing static conditions of poverty, low living standards, and economic backwardness. Comparative data assembled by political scientists also indicate a positive correlation between political instability and rapid economic growth.[39] What then is the connection between poverty and oppression, economic development, and revolution?

To this question a political scientist, J. C. Davies, has proposed an answer in the form of a model that combines features of both Marx and Tocqueville.[40] It is based on the view that a situation most favorable to revolution exists when a prolonged period of economic growth is followed by a short period of sharp reversal. New expectations, needs, and standards have then been created that are suddenly checked. With the J-curve, as Davies calls the downward swing, an acute discrepancy appears between mounting wants and expectations and the possibility of their satisfaction, which, in turn, establishes the potential for revolution. Revolution is accordingly seen as the effect neither of poverty and misery nor of improvement but of a certain sequence of the two. Davies has tried to fit his model to several cases, and although it cannot be accepted as a general explanation of revolution, it is clearly pertinent to certain revolutions like the French, which began when contraction, bad harvests, and famine succeeded a secular phase of growth. Lawrence Stone has pointed out that the J-curve can also apply to other sectors beside the economic. Thus trouble may arise if a phase of liberal governmental concessions is followed by a phase of political repression or if a phase of easier entry into the elite is followed by a phase of aristocratic reaction and closure.[41]

A much broader approach founded on similar premises has been

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of oppression is only one of the conditions of revolution. There are also other conditions, including the development of class consciousness in the oppressed. As regards capitalism, Marx gives a concise analysis of the "law" of capitalist production leading to the deterioration of the worker's position in *Wage-Labour and Capital* and in *Value, Price and Profit* both in *Selected Works* (New York, n.d.), I.

[39] Cf. M. Olson Jr., "Rapid Growth as a Destabilizing Force," and Ivo K. and Rosalind L. Feierabend, "Aggressive Behaviors Within Polities, 1948-1962," both in J. C. Davies, ed., *When Men Revolt and Why* (New York, 1971).

[40] J. C. Davies, "Toward a Theory of Revolution," in Davies, ed., *When Men Revolt.*

[41] Stone, "Theories," 172.

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recently developed in a book by Ted Gurr, *Why Men Rebel.*[42] Gurr's point of departure is the theory of relative deprivation, which may be defined as the discrepancy that individuals perceive between what they expect and what they are likely to obtain. Relative deprivation may exist either because value expectations increase faster than value capabilities or because value expectations remain the same while value capabilities decline. In both cases men will feel anger, frustration, and discontent. If relative deprivation is strong enough and becomes focused on political objects, a situation arises that may lead to violence and revolution. Whether revolution does occur or not depends on a number of variables, including the scope and intensity of the relative deprivation and the actions taken by the regime. Gurr has elaborated the logic of this analysis with great thoroughness. Thus the expectations subject to relative deprivation are not necessarily limited to the economic and the material. The expectations may be about power and political participation or status, personal development, and belief. Gurr attempts to specify the diverse causes of relative deprivation and to explicate the conditions affecting both regimes and dissidents that may culminate in revolution. The analysis gives close consideration to the role of legitimacy, to the effects of violence under various circumstances, and to the determinants of support for rebels and regimes. It also suggests methods such as opinion surveys to measure some of the variables involved.

Gurr's work is of exceptional interest both in its range and precision. The main question that it raises concerns its fundamentally psychological formulation and treatment of the problem of revolution. The theory of relative deprivation is derived from the studies done by social psychologists on frustration and aggression. It thus begins by locating the cause of dissatisfaction and discontent in the individual. Starting there, it generalizes the process to account for many people's discontent and then by a further progression to the likelihood that these same people will become violent, will direct their violence against political targets, and eventually engage in revolution. The focus is accordingly on the side of personal experience leading to the formation of discontent.

There are several important drawbacks to this view. Apart from the practical difficulties in determining the scope and intensity of

[42] Ted Robert Gurr, *Why Men Rebel* (Princeton, 1970).

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relative deprivation both in the present and even more in earlier periods where the data are scanty, a very long distance separates discontent from revolution. In order for the one to lead to the other, many factors must intervene, such as developments in the social and economic structure, conflicts and changes in the political system, the emergence of ideologies, and the like. Such factors are crucial to the dynamic causal process of revolution. Furthermore, they are not attributes of states of mind, but properties of political and social situations or systems. Gurr's theory, however, cannot take account of structural developments or political conflicts except as they are related to the psychological phenomenon of relative deprivation. This inability considerably diminishes both the significance and the possibility of analyzing adequately nonpsychological factors. For that reason, although the relevance of a psychological approach is undeniable, it seems to be in itself a limited and incomplete one.

In explaining revolutions, it is obviously necessary to distinguish long-range preconditions and processes from immediate precipitators which, by chance, act to release revolutionary situations. Only the preconditions can be incorporated into causal theories. Any attempt, however, to elevate a particular set of preconditions into general causes runs into difficulties. Harry Eckstein, a political scientist, has classified the commonly advanced hypotheses dealing with the causes of revolution, or, as he prefers to call it, internal war.[43] Some emphasize conditions of the social structure, like too much or too little social mobility, the emergence of new social classes, and the inadequate circulation of elites. Others emphasize ideological conditions, like the conflict of belief systems and social myths or the existence of corrosive philosophies and unrealizable values. Others stress economic conditions, such as poverty and exploitation or rapid growth. Still others emphasize political conditions such as government oppression, division within governing classes, or the excessive tolerance of alienated groups. With such an abundance of partially inconsistent hypotheses, any of which might yet be valid in a particular case, the obstacles to a causal theory are apparent.

Eckstein offers some suggestions to improve the explanation of the preconditions for revolution. First, he points out that histori-

[43] Eckstein, "Etiology of Internal Wars."

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ans and theorists have tended to pay more attention to rebels than to incumbents, but that both should receive equal consideration. This can hardly be gainsaid in view of the importance of elite behavior in creating the preconditions. Some writers, like Pareto for instance, have even held that revolutions are largely due to changes in elite characteristics, leading to estrangement from non-elites, incompetence, and the failure of nerve.[44] Second, any explanation should take into account behavioral as well as structural factors. This is contrary to the common approach, which has tended to concentrate on the structural or so called objective conditions of revolutions. The same conditions, however, may and do produce different results because orientations, values, and attitudes mediate between the social environment and action. Hence, culture, ideology, and the role of intellectuals in maintaining the values of legitimacy must also be stressed in the formation of preconditions. Third, explanations should strive for broad propositions describing general social processes which can apply to a large variety of specific preconditions. This last suggestion is no doubt valid, but at the same time there is also the danger that such propositions may become vacuous or trivial through their very generality.

Based on these ideas, Eckstein has proposed an explanatory model containing eight variables which in combination establish the presence and extent of the preconditions for revolution. The negative variables working against revolution are the facilities (especially for coercion) available to incumbents, effective repression, adjustive concessions, and diversionary mechanisms. The positive variables working in favor of revolution are the inefficiency of elites, disorienting social processes, subversion, and the facilities for violence available to insurgents. With this paradigm, Eckstein holds that it is possible to assess the potential for revolution and the balance between the favoring and countervailing factors.

Eckstein's model seems a useful one in its ability to incorporate different sets of particular preconditions. At the least it provides an inventory of factors to be checked off in explaining revolutions. It would therefore be of considerable interest to see it applied to an historical case in order to test its fit to the complexity and untidiness of the events.

[44] Cf. V. Pareto, *Sociological Writings*, ed. S. E. Finer (London, 1966), 55-58.

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There are a number of other angles from which recent social-science scholarship has attacked the problem of revolution. For instance, in an historical monograph on the Vendée, Charles Tilly, a sociologist, has based an analysis of a counterrevolution on the effects of urbanism and market penetration in a region of traditional subsistence agriculture.[45] A noticeable trend has also been to subsume revolution under theories of violence.[46] The assumption implied in such studies is reflected in Gurr's work as well as in the attempts of political scientists to establish precise indices and measures of conflicts within states.[47] The identification of violence and revolution is a mistake, however, for while the use of violence is certainly one of the defining characteristics of revolution, the two are not identical phenomena. A people may be prone to violence, like Americans, without necessarily being revolutionary. Many acts of collective or political violence—such as banditry, labor strife, lynchings, food, race, and antiforeign riots—may bear no relation to revolution or to the formation of the preconditions for revolution. A great deal of violence, moreover, proceeds from agencies of the state both in their "normal" activities and in repressive actions, such as those carried out against peaceful demonstrations with no revolutionary import. Also, the major kind of collective violence is carried out by governments when they make external war. Hence, though revolution and violence have some common features, they are different phenomena requiring different theories for their elucidation.

Revolution is often discussed in connection with theories of modernization. The study of modernization has become one of the preoccupations of current political science and focuses on the problem of stability in societies undergoing rapid social and economic change, its interest being for obvious reasons concentrated mainly on the contemporary scene. Modernization itself is sometimes regarded as a revolution rather than as the possible outcome or char-

[45] Charles Tilly, *The Vendée* (Cambridge, 1964); cf. Charles Tilly's "The Analysis of a Counter-Revolution," *History and Theory*, III (1963), 30-58.

[46] Cf. the discussion in Bienen, and in H. L. Nieburg, *Political Violence* (New York, 1969).

[47] Cf. Ivo K. and Rosalind L. Feierabend and Ted Robert Gurr with Charles Ruttenberg, *The Conditions of Civil Violence: First Tests of a Causal Model* (Princeton, 1967); see also some of the papers in Hugh Davis Graham and Ted Robert Gurr, eds., *Violence in America: Historical and Comparative Perspectives* (New York, 1969).

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acteristic of a certain type of revolution. Thus one writer speaks of "the revolution of modernization," which he calls "the first revolution in history to set a new price upon stability in any system of society; namely, an intrinsic capacity to generate and absorb continuing transformation."[48] This paradoxical observation repeats the familiar mistake of making revolution synonymous with any rapid, large-scale change. It also assumes that a society that has achieved modernization will be free of violent change and can remain stable even if transformation is continuous and incessant. Both assumptions are doubtful and pertain more to the utopia of modernization theorists than to the real world.

Probably the outstanding work devoted to revolution in the context of modernization is Samuel P. Huntington's *Political Order in Changing Societies*, an impressive synthesis of the conditions of stability and instability. As I have previously remarked, Huntington restricts the definition of revolution exclusively to transformations of the most sweeping character which are effected by violence. His argument is that social and economic changes such as urbanization, industrialization, the spread of literacy, of education, and of communication facilities, and so on lead to a rise in political consciousness, the mobilization of new groups into politics, and the multiplication of political demands. Traditional societies launched on the process of modernization usually lack the political institutions and organizations capable of bearing the heavy new strains put on them. There is consequently an imbalance between socioeconomic growth and the political capacity to assimilate the new forces mobilized in the process. The result is instability, disorder, and, in certain cases, revolution. Accordingly, in Huntington's view, revolution is an aspect of modernization. It is very unlikely to occur either in highly traditional societies or in highly modern ones and is least probable in both democratic and communist political systems because of the capacity of each to absorb new groups. Huntington also distinguishes a Western and an Eastern pattern of revolution. In the Western pattern—of which the French, Russian, and Mexican Revolutions are instances—the decay of political institutions and the collapse of the old regime precedes the process of political mobilization and the

[48] Manfred Halpern, "The Revolution of Modernization in National and International Society," *Revolution: Nomos*, VIII (New York, 1966), 179.

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creation of a new order. In the Eastern pattern, which includes the Chinese and Vietnamese Revolutions, political mobilization and the shaping of new institutions come first and the overthrow of the existing order last.[49]

I will not attempt to assess this challenging thesis in detail, but instead will merely express some critical reservations. Huntington's theory is based on a three-stage classification of societies as traditional, transitional or modernizing, and modern. While these categories are useful for some purposes, they are much too large and undifferentiated to describe the course of development as the historian sees it. To place French, Russian, and Mexican societies, for example, within this classification and then to treat the revolutions as the effects of modernization adds little to our understanding of them. The contrasts between the societies, economies, cultures, and states concerned are in many respects far too wide to justify such a procedure. It is not clear, moreover, why there should be two patterns of revolution, since Huntington's model appears to require that the political mobilization of new groups must always precede the breakdown of the traditional regime. With only one pattern, however, difficulties arise because certain revolutions cannot be made to fit the historical sequence. Furthermore, throughout, the analysis is continually open to the danger of advancing by a circular argument. Instead of consistently treating revolution and modernization as variables between which, in every case, an empirical connection must be established, Huntington sometimes joins them by definition. Thus, in explaining why China underwent a revolution and Japan did not, he tends to judge whether the political stresses resulting from modernization were severe enough to cause a revolution merely by the fact that a revolution did, indeed, occur.

This problem poses the larger issue of whether modernization, as such, can be accepted as the cause of revolution. There is, to be sure, a good deal of evidence that rapid social and economic change is apt to produce instability.[50] Modernization itself, nevertheless, is merely a name for an ensemble of complex processes.[51] Unless

[49] Huntington, chaps. 5-6.

[50] Huntington, chap. 1, and the references cited in n.39 above.

[51] The difficulties in defining modernization are well known; cf. Richard Rose, "Modern Nations and the Study of Political Modernization," in Stein Rokkan, ed., *Comparative Research Across Cultures and Nations*

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these are disentangled and their effects separately tested and verified, it is difficult to see how reliable conclusions can be drawn as to their connection with revolution. If we follow Huntington, modernization leads to revolution via the political mobilization it engenders. "Revolution," he writes, "is the extreme case of the explosion of political participation."[52] But while it is true that great revolutions are accompanied by unprecedented mass involvement in politics, how can the fact of modernization account for the support of a revolution by groups already involved—by intellectuals, bourgeoisies, and dissident aristocracies, for example, as has often been the case? The alienation of participants in political life, and the withdrawal of their support from existing regimes, is frequently of profound importance in the inception of revolution, yet it is a phenomenon on which Huntington's theory throws little light.

A common feature in explanations of revolutions is to attribute them to some incompatibility or stress within the ongoing institutions and processes of society; the most general theory of this kind has been developed by Chalmers Johnson.[53] Johnson's starting point is the model of a functioning social system in a state of equilibrium. Equilibrium in this case does not mean the absence of change but rather synchronization between the different sectors of the system as change proceeds. Here Johnson is largely dependent for his ideas on the sociological theories of Talcott Parsons concerning the requirements of a functioning social system. The process of revolution accordingly begins with disequilibrium and dysfunction. These may ensue from changes in both values and the environment and such changes may originate either from within the system or be introduced from without. Dysfunction signifies a lack of integration between the sectors or subsystems of the social system as a whole. It occurs when the change taking place is too much for the adaptive and adjustive processes of the system. If dysfunction is not dealt with by purposeful measures to reestablish equilibrium, then it will multiply with widening ef-

(Paris, 1968). On the diverse components of modernization, cf. Daniel Lerner, "Comparative Analysis of the Processes of Modernization," in Rokkan. A general treatment is Cyril E. Black, *The Dynamics of Modernization* (New York, 1968).

[52] Huntington, 266.

[53] See *Revolution and the Social System* and *Revolutionary Change*.

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fects of disorientation and protest within the society. At this point the action of elites is crucial. If the elite relies on force to cope with the pressures of dysfunction, the result will be what Johnson calls a power deflation. On the other hand, if the elite fails to institute policies capable of reintegrating the system, it will suffer a loss of authority. Sheer intransigence on its part may also be fatal. Multiple dysfunction and elite inefficiency create a revolutionary situation. What then makes revolution highly probable or certain, according to Johnson, is the intervention of some unforeseen triggering factor or accelerator. This could be an event such as a defeat in war or any other incident that undermines an army's loyalty to a regime and persuades revolutionaries that they have the chance to win power. Johnson's formula can therefore be summarized as follows: change plus disequilibrium plus multiple dysfunction plus elite failure plus an accelerator lead to revolution.

To account for the varieties of revolution, Johnson has also devised a classification based on four criteria: the targets of revolutionary activity, whether government, regime, or society; the identity of the revolutionaries, whether elites, masses, or both; the goals of the revolutionary ideology, whether reformist, nation-building, eschatological, etc.; finally, whether the revolution is a spontaneous or a calculated movement. With the help of these criteria, Johnson then distinguishes six types of revolution. The first is the jacquerie, a spontaneous peasant outbreak that does not challenge the legitimacy of the regime and appeals to traditional authorities for redress of grievances. The second type is the millenarian rebellion, which may be actuated by the same grievances as the jacquerie, but aims at a totally new order with the expected aid of supernatural forces. The third type is the anarchistic rebellion, which is a reaction against change or modernization and seeks to reinstitute an idealized older order. The fourth type, which is also the rarest, is the Jacobin-communist or great revolution and is the one that leads to the most far-reaching changes. The fifth type is the conspiratorial coup d'état, an attempt by a small elite to introduce social change by violence. Finally, the sixth type is the militarized mass insurrection, a creation of the twentieth century, which is a people's war of guerilla insurgency led by a dedicated elite and inspired by nationalist and communist ideologies.

This typology of revolution is the best that has yet been suggested and can be useful in discriminating particular revolutions,

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which may, of course, possess characteristics of more than one type. It cannot, however, be regarded as complete. There is no place in it, for example, for the religious-civil wars of the sixteenth and seventeenth centuries led by aristocracies nor for the separatist rebellions or urban revolts of the same period. Another flaw is that the Jacobin-communist type of revolution is misleadingly named, since some great revolutions in this category, like the Mexican Revolution or the Nazi conquest of power, were neither Jacobin nor communist. One might also consider whether what Johnson calls the militarized mass insurrection does not really belong to the type of great revolutions and differs only in making use of new political and military strategies.

Turning from this typology to Johnson's model as a whole, it represents the most ambitious attempt made thus far to apply social-systems theory to revolution. Its principal defects are due to its too abstract and overly general character. The assumption, for instance, that a nonrevolutionary society is a system in equilibrium is one that arouses considerable skepticism. Has any such system ever existed? Surely it assumes that societies are far more integrated than they possibly can be. A more plausible view is that imperfect integration and some degree of disequilibrium are characteristic of every social system. But such talk is, in any case, unilluminating in the absence of definite criteria for specifying equilibrium and disequilibrium. The criteria that Johnson proposes for this purpose are unfortunately of little help. Thus, equilibrium is described as a condition of synchronization between values and the social division of labor.[54] Such a formula, however, suffers from extreme vagueness unless clear indicators are given by which to identify and distinguish this condition from its opposite. In the case of disequlibrium, Johnson mentions somewhat more precise criteria, such as a rising suicide rate or increased ideological activity.[55] The drawback here, however, is that we have no reason to accept these factors as *necessarily* reflecting disequilibrium unless we first accept the theory they are supposed to confirm. Problems arise as well with regard to the conception of dysfunction. Every social system contains many instances of poor governmental performance, elite failure, and out-of-date institutions. Since these are all presumably dysfunctional, how to recog-

[54] Johnson, *Revolutionary Change*, 56, and chap. 3.
[55] *Ibid.*, chap. 6.

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nize dysfunction as a special condition resulting from disequilibrium becomes a puzzling question. Considerations of this kind suggest that equilibrium, understood even as an ideal condition to which actual systems are merely in some approximate relation, is not a good foundation on which to erect a theory of revolution. A final point is that Johnson's work concentrates so heavily on the social system that it is relatively uninformative about the specific conflicts present in revolution, the genesis of revolutionary opposition, or the actions undertaken by governments and rebels. In placing himself on the heights of general theory, he has not avoided the danger of making his fundamental propositions so expansive that they verge toward emptiness and thus fail to entail the concrete correlatives by which their adequacy can be tested.

IV

After this review of theories of revolution, the main conclusion to be drawn is that the subject is in a lively but disorderly state. A number of promising ideas have been propounded, but they do not fit into any coherent scheme. Whether a general theory of revolution is attainable is a question one might well wish to ask, in view of the disagreements and lack of success of past and present efforts. Of course, no answer to this question can be given on *a priori* grounds, and every theoretical contribution should be assessed on its merits. Nevertheless, I admit to considerable skepticism in the matter. As an historian, I am inclined to believe that above a certain level of generality social-science theory is too remote from reality to be either very interesting or useful. A global approach toward the problem of revolution seems to me, therefore, less likely to be profitable than one that strives, for instance, to clarify the different types of revolution and to develop a model of the causal process of each type. Yet despite the reservations historians are apt to feel about the utility of grand theory, they need to take account of whatever theories are available and to try to use and improve them. In the present position of history, the union of theoretical and comparative investigation with the more traditional methods of inquiry is one of the most important lines of advance toward a deeper understanding of the past.

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The Gamut from A to B: The Image of the Black in Pre-1915 Movies

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The outrageous and shabby treatment that blacks have received at the hands of the American motion picture industry has become common knowledge in recent years. The matter has been aired thoroughly on television and explored fully in newspapers and magazines. But although only recently drawn to the American public's attention, the fact that the movies until just a few years ago either completely ignored blacks or, with rare exceptions, stereotyped them in demeaning and/or condescending roles has been a subject of discussion and condemnation for a long time, especially in the black community.

As long ago as 1914, when the movies were still in their infancy, the respected black journalist Lester Walton strongly denounced the industry for "ridiculing" and "continually representing the race at its worst." Arguing that the movies could be an important educational force for improved relations between black and white America, Walton declared that the duty of the industry should be "to emancipate the white American from his peculiar ideas" about the black community and not to further "incorrect notions of the colored American . . . hurtful to both races."[1]

[1] Lester Walton, *The New York Age*, May 23, 1914, p. 6. In commenting on the particular movie which occasioned his article, Walton touched on a

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Walton's comments were echoed over the years by those, both blacks and whites, concerned with correcting the distorted image of the black presented by the American movie industry. Among the most unusual and poignant restatements of these criticisms is one made in early 1940 by Andy Razaf, a noted black lyricist (and contributor to shows like *Shuffle Along*):

I always leave a picture show disgusted—Holy Smokes!
Don't they know colored people are just like other folks?
Why do they always think that all we know is sin and strife,
Tho' we have many of our race in every walk of life?
Are Hollywood producers mindful of their harmful acts,
Or are they just plain ignorant and do not know the facts?
They show us all as comics, gangsters, and slowpokes,
Don't they know colored people are just like other folks?[2]

In 1944, Lawrence Reddick, then curator of the Schomburg Collection of Negro literature of the New York Public Library, took a more scholarly approach to the issue. He surveyed one hundred American films from the beginning of the silent period to the 1940s and found three-quarters of these films to be anti-Negro. Even though the "overwhelming desire" of the Negro people is to be treated on screen "like everybody else," he found that there had been only a "limited range" of Negro characterizations in the movies: there was a "ceiling" above which blacks portrayed in films rarely if ever rose, and "this ceiling on screen is lower than the ceiling for the Negro in American life itself." Invariably, according to Reddick, the blacks in these films and others exhibit qualities of "ignorance, superstition, fear, servility, laziness, clumsiness, petty thievery, untruthfulness, credulity, immorality, or irresponsibility with a predilection for eating fried chicken and sliced watermelon."[3]

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subject that has produced more recent controversy in other areas. He found it "strange" that Jewish film producers should find it "so agreeable to degrade the colored American when the Jews throughout the country" were engaging in "a campaign against stage caricaturing of their own."

[2] Andy Razaf, "A Colored Movie Fan," *New York Amsterdam News*, Jan. 6, 1940, p. 16.

[3] L. D. Reddick, "Educational Programs for the Improvement of Race Relations: Motion Pictures, Radio, The Press, and Libraries," *Journal of Negro Education*, XIII (1944), 367-89. Reddick points out that the various stereotypes of the black in the American mind were sometimes contradictory, but that on the whole they supplemented each other; he listed the principal aspects

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Reddick's discussion of the development of Negro stereotypes in American film is useful, but, like many comments on the subject before and since, it dwells on the historical importance of the portrayal of the black in the film *The Birth of a Nation* and barely touches on what happened in the two decades prior to the general release of this film in the spring and summer of 1915. For many reasons, not the least being its treatment of blacks, *The Birth of a Nation* is an important milestone in the social history of the United States as well as in the development of world cinema. Yet it must be remembered, and often is not, that the caricatures of the black which this film presents did not like Athena spring full-blown.

Certainly the tenor of the period from the 1890s (when movies first were made) to 1915 did not encourage serious or dignified treatment of the black by motion pictures. Racial libels abounded in all areas of life, and the public's attitude probably victimized blacks more than any other group. It was, as C. Vann Woodward has pointed out, an era in which "the extremists of Southern racism probably reached a wider audience, both within their own region and in the nation, than ever before."[4] Newspapers and magazines used the words "negro," "nigger," "darky," "colored," and "coon" indiscriminately and often interchangeably. As late as World War I, the *Cincinnati Enquirer* carried a headline "Yah Suh! Black Boys Are Happy" above a story on black troops in France.[5] The literature of the day viciously lampooned the black, glorified the plantation tradition, rationalized lynchings, explained away inequitable treatment of blacks, and as Rayford Logan concluded after surveying the field "continued to mirror preponderantly hostile attitudes to the Negro."[6] A staff writer of *Moving*

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of the black image as: the savage African, the happy slave, the devoted servant, the corrupt politician, the irresponsible citizen, the petty thief, the social delinquent, the vicious criminal, the sexual superman, the superior athlete, the unhappy semi-white, the natural-born crook, the natural-born musician, the perfect entertainer, the superstitious churchgoer, the chicken and watermelon eater, the razor and knife "toter," the uninhibited expressionist, and the mental inferior.

[4] C. Vann Woodward, *Origins of the New South* (Baton Rouge, 1951), 352.

[5] Quoted in Rayford W. Logan, "The Negro as Portrayed in Representative Northern Magazines and Newspapers," in Barry Schwartz and Robert Disch, eds., *White Racism* (New York, 1970), 395.

[6] Rayford W. Logan, *The Betrayal of the Negro* (New York, 1965), 275.

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Picture World, a film-industry trade journal, who described himself as being without prejudice, at the same time wrote, "never again will I go to a theatre, knowing that fellow citizens of African descent form part of the audience [because] . . . so many of the colored people are like children and insist on laughing at the pictures, no matter how tragic they may be."[7]

The depiction of blacks in early American movies, however, probably was influenced most profoundly by the treatment of the black on stage and in the minstrel shows and vaudeville. The minstrel shows—whose performers appeared with faces blackened by sooty burnt cork makeup—followed an elaborate ritual in their burlesque of Negro life in the Old South. Already well established before the Civil War, they succeeded in fixing the black man in the American consciousness as a ludicrous figure supposedly born, as one show-business history puts it, "hoofing on the levee to the strumming of banjos."[8] The stereotyped black man was prone to frenzied dancing, shiftlessness, garish dress, gin tippling, dice shooting, torturing the language, and, inevitably, was addicted to watermelon and chicken, usually stolen. So fixed was this tradition that in the latter part of the nineteenth century when black professional companies began to present minstrel shows, the troupes' performers found it necessary to adopt almost completely the minstrelsy ritual, even to the use of burnt cork makeup. The result was an imitation of an imitation, what James Weldon Johnson called "a caricature of a caricature."[9]

During the 1890s black performers began to break with the minstrel formula, and they had some success in all-Negro musical shows and in vaudeville. The cakewalk, described by Langston Hughes as "the first dance of Negro origin to sweep this country," made its mark at this time.[10] But black performers, even the most talented, could not escape the "fantastic artificiality" of the caricature in which they had been imprisoned. Bert Williams, for in-

[7] "Random Shots of a Picture Fan," *Moving Picture World,* Nov. 18, 1911, p. 553.

[8] Abel Green and Joe Laurie Jr., *Show Biz: Variety from Vaude to Video* (New York, 1951), 8.

[9] Quoted in Loften Mitchell, *Black Drama* (New York, 1967), 31-32.

[10] Langston Hughes, "The Negro and American Entertainment," in John P. Davis, ed., *The American Negro Reference Book* (Englewood Cliffs, N.J., 1966), 831.

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stance, was an extraordinarily gifted man whose speech was impeccable; on stage the erect, lightskinned, distinguished-looking Williams appeared as a slouching, dialect-speaking, inept shuffler wearing a burnt cork mask he hated. Williams and his partner of many years were billed as "The Two Real Coons." Around the turn of the century, the "coon" concept of the black found different kinds of expression on stage. The sentimental "darky melody" gave way to the raucous and bawdy "coon song." Often performed by whites in blackface these songs and others such as "You May Be a Hawaiian on Old Broadway but You're just Another Nigger to Me" as well as the so called "coon shows" like *The Coon and The Chink, Jes' Lak White Folks, Bandanna Land,* and *Dat Famous Chicken Debate* ("Resolved that Stealing Chickens Ain't No Crime") further fixed the stereotype in the public mind.

In the contemporary theatre blacks did not fare much better. During the final decades of the nineteenth century and the first years of the twentieth, when attempts at photographic reality dominated the stage—of one 1897 production it has been said that "the Chinese quarter of San Francisco was reproduced almost to its smells"[11]—blacks were invariably portrayed by white actors in blackface. And at a time when many of the more popular stage vehicles were Civil War melodramas, plays about the antebellum South, and versions of *Uncle Tom's Cabin,* the "real" stage black was either the comic stooge or a faithful retainer. The stooge differed little in detail from the caricature popularized in the minstrel shows and in vaudeville. The retainer presented a different image, but one which also failed to suggest that the black is a human being with individual traits. Loyal, uncomplaining drudges, the retainers were "the plantation uncle, the broad bosomed mammy . . . , kindly tyrants of kitchen and garden, hard working, good natured, quaintly humorous, obsequious, affectionate, and faithful."[12] For them, even in a play set after the Confederacy's defeat, emancipation made no difference—they continued to serve.

Other important stage stereotypes of the black at this time were the "brute" and the "tragic mulatto." The former appeared infrequently, but, interestingly enough, in the theater the black-as-

[11] Mordecai Gorelik, *New Theatres for Old* (New York, 1940), 161.

[12] Farrin Belcher, "The Place of the Negro in the Evolution of the American Theatre, 1767-1940" (Ph.D. diss., Yale Univ., 1945), 1-5, 48-56.

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threat was not limited to menacing males. Just after the turn of the century the young, then well known Northern playwright Eugene Walter, describing the characters in his drama *The Easiest Way*, wrote that "particular attention . . . should be called to the character of the negress Annie, who . . . must not in any way represent the . . . traditional 'mammy.' She is cunning, crafty, heartless, sullen. . . . The actress who plays this part must keep in mind its innate and brutal selfishness."[13] The mulatto, as presented on stage, served to reinforce popular notions about white superiority. A character's black blood, however limited it might be, was the cause of trouble. Implicit when not painfully explicit was the sentiment that the character's white blood was responsible for any redeeming features he might have. In the "mixed blood" plays, a tragic ending for the mulatto was avoidable only if just before the curtain rang down it became clear that the character was really all white, for in these plays the merest drop of Negro blood was a taint from which there was no redemption. The theatrical alternative to viewing the black as ridiculous was to view him as sinister.

These caricatures of the black in the theater, musical shows, and vaudeville found ready acceptance in American motion pictures as they rapidly developed into one of the most important entertainment media in the country during the years before World War I. By 1908, the movie industry's gross revenues exceeded those of the theater and vaudeville combined, and it was estimated that more than four million people a day went to the "flickers." Yet the Kinetoscope, a peep-show device which allowed one person at a time to see pictures move, had not been invented until 1889. By the end of the 1890s, in addition to the common use of peep-show devices, crude projectors and screens had been perfected, and commercial public showing of movies had begun. The usual bills of fare were comedies of some sort and travelogues, news reports, documentaries, and sport films—many of them faked—each two to three minutes in length and strung together to make a show. After the turn of the century, the industry increasingly began to produce motion pictures that told some kind of story, although

[13] Eugene Walter, *The Easiest Way* in Thomas Dickinson, ed., *Chief Contemporary Dramatists, Second Series* (Boston, 1921), 174. The best known example of the brute on stage is probably the dramatization of Thomas Dixon's novel *The Clansman*, which was filmed as *The Birth of a Nation*.

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often a primitive one, and it took time for the plots to evolve into something more than a peg on which to hang a slapstick sequence or a horseback chase. As technical and artistic innovations matured the medium, plots became more detailed and complicated, movies became longer, and by 1915 motion pictures increasingly were story films of feature length. These changes in the American film industry, however, made little difference in the treatment of the black in its productions.

During this period except for the very early movies—which often were no more than filmed vaudeville turns—the screen Negro, as approximated by whites in blackface, was usually wooly-headed, thicklipped, and very darkskinned. Occasionally, in films with large casts, blacks are found in very subordinate roles or as part of the *mise en scène*. At least one social scientist has contended that "the white man in blackface serves the psychological function of reducing audience anxieties that might occur if real Negroes were used, especially in scenes of overt and covert sexual nature or when the Negro gets the upper hand over the white man."[14] But it is more likely that white actors were used because the filmmakers of the time, a crude but pragmatic lot on the whole, accepted the prevailing beliefs about the abilities of blacks and proceeded accordingly. Certainly their treatment of blacks indicates that they shared the prevailing prejudices. The Gene Gautier Feature Players, filming in Jacksonville, Florida, boasted in their publicity for the 1913 film *Mystery of the Pine Tree Camp* that for one scene the company had borrowed a real Negro convict from the authorities, given him "a start of fifteen minutes over the dogs," let the bloodhounds loose, and had him treed in short order. The Lubin Company group in Jacksonville, publicizing its production films there, reported how the directors of the operation attempted to bribe local blacks used in the films and other blacks into becoming grave robbers; Mr. Hotalong and Mr. Montgomery, said a correspondent, "have fine hunting in their little comedy sketch entitled 'Want to earn two dollars?' "[15]

Toward the end of 1912—partly for economic reasons, partly as a commercial gimmick, and partly to attract a larger black audience—more companies began to use black players. But this occa-

[14] Robert Williams, "Stereotypes of Negroes in Film," *Vision*, I (1962), 69.
[15] *Moving Picture World*, Apr. 5, 1913, p. 53; Jan. 11, 1913, p. 139.

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sional use of black actors and actresses did not present any real opportunity for such stellar performers as Bert Williams. He remained a victim of a cruel racism that allowed him to be proclaimed the hit of the Chicago company of the *Follies of 1910* although not one newspaper in that city would carry his picture. In 1914 Williams did make a number of one-reelers of his most successful stage turns, but these movies although well done found little market except in movie houses patronized by blacks. All the distributors approached gave the same apologetic response: they would like to use the films, but "the southern territory would resent and would not exhibit the pictures of a Negro star."[16] This, apparently, was not the response made to the companies which utilized their black performers in the often vicious comedies about what was imagined to be Negro life.

John and Mattie Edwards as well as some other blacks appeared in a series of such films for the Lubin Company, which advertised in the black press for "characteristic stories . . . for the Negro stock company of the comedy section of the Lubin Company. All ideas will be paid for. . . ."[17] The Historical Feature Film Company of Chicago, utilizing the talents of what it called "the well-known negro comedians Bert Murphy, Frank Montgomery, and Florence McClain," made a number of movies such as *Money Talks in Darktown*.[18] The 1914 filming of *Uncle Tom's Cabin*, for which the production company claimed greater authenticity than the earlier movie versions, had in the title role a 72-year-old black actor, Sam Lucas, described by *The New York Age* as the "dean of the colored theatrical profession." Lucas received good notices in a trade journal, but the reviewer's attitude toward the other black actors in the movie is summed up by his praise for the director on his handling of the film, saying that it was "all the more notable by reason of the fact that . . . Daly was using many colored players. . . ."[19] And in notices of other films in which blacks appear as more than just background there was often a tone of incredulity: "a picture of colored people played by real colored actors . . ." is how one such review begins.[20]

[16] Mabel Rowland, ed., *Bert Williams, Son of Laughter* (New York, 1923), 150.
[17] *The New York Age*, Mar. 5, 1914, p. 6; Apr. 16, 1914, p. 6.
[18] *Moving Picture World*, Feb. 6, 1915, p. 898.
[19] *Ibid.*, Aug. 22, 1914, p. 1097.
[20] *Ibid.*, Oct. 5, 1912, p. 82.

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Whether or not portrayed by blacks, black characters appear only in certain kinds of films among the hundreds of motion pictures produced between the mid-1890s and 1915, and there rapidly developed stereotyped images of the movie black. Only a few films avoided stamping their black characters with this stereotype. An early example of such a motion picture, for all its fanciful exaggeration and its unfortunate title, is *The 'Gator and the Pickaninny*, a 1903 Biograph film in which a black man with an axe unhesitatingly attacks an alligator that has swallowed a small black boy; as a result the boy, Jonah-like, is restored whole.[21]

In a significant number of films, including the comedies, the possibility of violence or at least strong action against white persons or property by black characters is implied, but only rarely is such violence shown. Examples include the kind of incidental action which takes place in the 1914 Warners drama *A Mother of Men*, in which a slave kidnaps one of the master's sons, or the 1910 Kalem sob story *For A Woman's Honor*, in which a black passerby steals money out of the handbag of an old white woman who has had an accident. The 1904 Biograph comedy *A Bucket of Cream Ale* shows a black serving woman dumping a bucket of beer over a man who has thrown his glass of beer at her; this most unusual scene can be explained partly by the fact that the man involved is a stereotyped German, often the butt of ridicule in many early films. That even the black as a comic menace generally was found so little on the screen is probably, as one scholar points out, "due to the fear of whites that to portray the Negro [so] . . . was to affect a possibility of its realization."[22] And where there is conflict between black and white, the end result is always the same—white superiority. It makes no difference whether the film is a

[21] *The 'Gator and the Pickaninny* is one of the films from the Paper Print Collection of the Library of Congress. Before the enactment of the motion picture copyright law in 1912, film producers adopted the practice of making paper bromide contact prints of their films and depositing them with the Library of Congress Copyright Office. Over 3,000 of these prints have survived, and between 1953 and 1965 they were all transferred to 16mm film under the supervision of Kemp Niver, who developed a process for restoring the paper prints and putting them on projectable film stock. Films may be viewed and prints bought there. See Kemp Niver, *Motion Pictures from the Library of Congress Paper Print Collection: 1894-1912*, ed. Bebe Bergsten (Berkeley, 1967).

[22] Joseph Boskin, "Sambo: The National Jester in the Popular Culture," in Gary Nash and Richard Weiss, eds., *The Great Fear: Race in the Mind of America* (New York, 1970), 166.

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melodrama such as the 1914 Selig production *The Loyalty of Jumbo*, in which a number of hostile African natives are held off by a hardfighting white mother while her little daughter's pet elephant goes to fetch help, or a comedy such as the 1915 Vitograph movie *Some White Hope*, during the course of which Negro boxer Sam Bangford (obviously patterned on the noted and then active black boxer Sam Langford) is accidentally but firmly kayoed by Kid Limburger of Hoshkosh. But even such conflict is not often shown. For the most part films made from the 1890s to 1915 in which black characters appear emphasize the nonthreatening black. Among the first motion pictures made for commercial showing is *Washing of a Pickaninny by His Mother*, a comedy of the mid 1890s, which audiences reportedly "highly enjoyed."

Drawing their inspiration from the stage, motion pictures made during the 1890s usually present blacks (whether or not they are whites in blackface) in dance sequences. The 1894 Edison catalog of motion pictures available for peep shows, for example, lists *The Pickaninnies Doing a Dance*, and two other such motion pictures—one showing blacks doing a buck-and-wing, the other a cakewalk—which are described as "the best negro subjects yet taken" and "amusing and entertaining."[23] Another example is *My Ragtime Gal*, an 1899 Biograph motion picture for peep shows. The "dancing fool" caricature of the black even found expression in the first filming of *Uncle Tom's Cabin* in 1903; one scene begins with a long cakewalk sequence which has absolutely nothing to do with the preceding or following action. As the movies developed, the mere showing of blacks dancing became passé, although the movies accepted, as in the 1907 Essanay production *The Dancing Nig*, what a note on this film refers to as "a well-known fact among negroes . . . that a darky finds it impossible to keep his feet still whenever he hears the sound of music."[24]

The supposition that "they got rhythm" notwithstanding, the comic stooge and the faithful retainer were probably the most common images of the black in motion pictures prior to *The Birth of a Nation*. These images and the others which appeared on screen often overlapped—the faithful retainer was a dancing fool as well

[23] The entries in the Edison catalog are reprinted in Terry Ramsaye, *A Million and One Nights* (New York, 1964), 838-39.

[24] "Moving Picture Notes," *Show World*, Oct. 12, 1907, p. 16.

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or was used for the purposes of comic relief; they drew heavily on the stereotypes presented in the theater, minstrel shows, and vaudeville—but with an important difference. The movies added a larger-than-life dimension to these stereotypes, for the early motion pictures, short and without sound, depended on exaggeration even greater than that practiced in the contemporary theater to make clear what was happening on screen. Thus, an actor would not just show, say, humility; he would be a suppliant beyond the wildest dreams of Uriah Heep.

This style of playing only heightened the ludicrous aspects of the black as comic stooge in what one critic called "the coon subjects," films presenting blacks as objects of derision. The 1904 Biograph movie *A Nigger in the Woodpile* is a crude early example of such a movie. Because wood is being stolen from the farmer's shed he places dynamite among the kindling; then, to quote the company's plot synopsis, "in the next scene, a colored deacon, one of the shining lights in the African church, is seen making away with the wood; the loaded stick is, of course, put into the fire, and there is a terrific explosion . . ." which blows off the roof and enables the farmer to identify the culprit.[25] A 1910 advertisement for *C-H-I-C-K-E-N Spells Chicken*, an Essanay production, begins "Ah love mah wattah melon but Oh, You-OO Chicken"; chicken snatching is a common plot device.[26] The black man, usually dubbed Rastus in the title or in the advertising blurbs, generally is shown in these movies to be childlike, foolishly pretentious, shiftless except when gambling or chickens are concerned, clumsy, and vulgar. The black woman character, often played by a man, usually is obese and physically unattractive; if single, she is unreasonably flighty. These characterizations even found their way into the primitive cartoons of the day. Lubin's *A Barnyard Mix-Up*, made in 1915, for instance, deals with "chicken-thieving Rastus," who escapes the farmer's buckshot but is finally laid low by an axe, although he is resurrected in an unusual manner by an explosion of dynamite. The manner in which this cartoon is drawn can only be described as vicious.

The faithful black retainer appeared on screen early in American

[25] Kemp R. Niver, compiler, *Biograph Bulletins 1896-1908*, ed. Bebe Bergsten (Los Angeles, 1971), 207.

[26] *Moving Picture World*, July 2, 1910, p. 45.

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motion picture history. In 1903, movie pioneer Edwin S. Porter (probably best known for *The Great Train Robbery*) adapted and filmed a stage version of *Uncle Tom's Cabin*, including the cakewalk sequence mentioned earlier. But the servile black retainer did not become a screen regular until later in the decade with the increased production of story films, one of the most popular subjects being what a Kalem Company publicity release described as "the comfortable, unpretentious, slaveholding aristocracy of rural Dixie."[27] *His Trust* and *His Trust Fulfilled*, 1911 motion pictures by D. W. Griffith which form a two-part story, recount how "the old trusted body servant" stays with the family during the Civil War, saves the little girl and his late master's sword when Union soldiers fire the house, and after the war and the death of the mother sacrifices his meager savings to educate the girl and make it possible for her to marry well. At the conclusion of the second film, as the *Biograph Bulletin* describes it, "old George at a distance views the festivities with tears of joy streaming down his black but honest cheeks, and after [the newlyweds] . . . depart for their new home, he goes back to his cabin, takes down his master's saber and fondles it, happy in the realization that he has fulfilled his trust."[28] Old, alone, without funds, the only reward he receives is a handshake from the white lawyer through whom he had secretly paid for the girl's education and who earlier had ignored the black's proferred hand.[29]

These and other films by Griffith present his particular view of the South before, during, and after the Civil War, but his films are no different in theme from what the rest of the industry was turning out. In most of the films manumission or the abolition of slavery apparently are meaningless to blacks. The "mammy" usually is a blindly constant surrogate mother, neglectful of herself and her own when it comes to "missy" and the rest of the family. She and her male counterpart are gladly willing to accept blame that is not theirs to cover up for the master's children, to sell themselves into slavery in order to pay the master's gambling debts, and to sacrifice their lives not only in the furtherance of "missy's" ro-

[27] *Ibid.*, Jan. 18, 1913, p. 263.

[28] *Biograph Bulletin* quoted in Robert M. Henderson, *D. W. Griffith: The Years at Biograph* (New York, 1970), 109-10.

[29] *His Trust* and *His Trust Fulfilled*, prints in the Library of Congress Paper Print Collection.

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mance or "massa's" pecadilloes but also in support of the latter's fight for the Confederacy. Nor was the "Uncle Tom tradition" limited to films about the South. A variation of the faithful retainer character can be found in jungle adventure films such as the 1913 Selig *Terror of the Jungle*, in which the character "Nig" stays to face danger with the wife and daughter of the absent white man when all the other natives decamp and when he easily could save himself, or in the 1912 Edison *Queen for a Day*, in which the missionary family's black maid is compelled to become a native queen but manages through a ruse to save the family and the daughter's white sweetheart, escaping with them "back to civilization."

If the black character is employed at all, he is a menial. Typical of the so called "actualities" which initially dominated motion pictures and which to some degree remained popular even after the advent of story films is the Edison film showing native women coaling a ship in the West Indies.[30] From time to time a newsreel shows, for example, Cuba-bound black troops disembarking during the Spanish-American War, but at a time when significant numbers of blacks were finding their way into industry, on the whole, films of this type which include blacks show them as laborers and deal with such topics as banana harvesting in the West Indies, tobacco planting in Kentucky, and cotton growing in the South. The filmmakers' attitude toward such productions is epitomized by this statement from a publicity release for the 1913 Lubin documentary on coffee plantations in Jamaica: "The natives work very rapidly and wear little beyond the perpetual smile which is noticeable in the negro race."[31] In story films blacks typically are cooks, scrub women, servants of one kind or another. Jungle adventure movies show either ignorant, often vicious savages or servile black gun bearers and porters. In the years before World War I, blacks were becoming involved in factory work at an increasing rate, but although filmmakers (taking their cue from the prevalent industrial unrest) made a substantial number of movies dealing with labor strife, rarely are blacks shown at all in these films—except, perhaps, as servants of the managerial upper class. Even the depiction of blacks as servants or agricultural labor-

[30] *Native Women Coaling a Ship and Scrambling for Money*, print in the Library of Congress Paper Print Collection.

[31] *Moving Picture World*, July 19, 1913, p. 338.

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ers is often a device to establish the regional setting. In a movie such as the 1915 Kalem drama *Unfaithful to the Trust*, presented as a contemporary story, the black servants appear only when and because the family has gone South to live; they have nothing to do with the plot. Such, of course, is not the case with the many films laid in the South of the Civil War period; these frequently include as an integral part of the plot a devoted "mammy" or a faithful field hand or house servant.

The movie black often was portrayed in ways that tended to give the character humiliating or demeaning aspects. The 1914 Vitagraph film *A Florida Enchantment* is a complex comedy about sex changes caused by the swallowing of unusual seeds. The main character, Miss Travis, having suddenly become a man, "so that she may be valeted by a person of the proper sex," makes her Negro maid take some of the seeds, which the maid must accept. A 1910 Essanay Western begins with the heroine, assisted by her Negro helper Sam, doing the wash; a cowboy comes by and tries to force his attentions upon the heroine. She resists, and Sam (in the words of the production company's plot synopsis) "being a coward himself . . . runs out . . . to see if . . . help cannot be secured."[32] The hero of a 1913 drama *The Message in the Bottle* (presented by Majestic) finds himself captured by African cannibals who prepare to burn him at the stake. Deciding to have a last cigarette, he makes use of his pocket lighter; the awestruck natives immediately fall back in fear of this godlike creature and make him their chief (this scene was a cliché even then). In *The Thirteen Club*, a 1905 Biograph production, a Negro waiter is setting the table for the club members' dinner when suddenly he notices the death's head centerpiece; instantly he gets the shakes and the bugeyes. A turn of the century Edison human interest newsreel records what apparently is a watermelon-eating contest among four Negroes. The man who finishes first decides he wants more, takes another's watermelon, and a free-for-all scuffle ensues.[33]

The movie industry, like white America generally, gave little thought to the dignity of the black, as is revealed in films in which the comedy is based on confusion among the races, the humor, de-

[32] *Ibid.*, Apr. 2, 1910, p. 479.

[33] *The Thirteen Club* and *Watermelon Contest*, prints in the Library of Congress Paper Print Collection.

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risive and vicious, invariably at the expense of the black. The 1909 Edison slapstick comedy *Drawing the Color Line* derives its humor from the main character having his face blackened with burnt cork while drunk and the ensuing reaction to him by friends, barbers, tradesmen, servants, and family to whom he is just an anonymous black man. The *Moving Picture World* reviewer rather thoughtfully commented that this "picture shows very graphically how merely putting a little black on a man's face changes status" but added that the "subject is one that never fails to arouse much merriment."[34] An earlier example of this kind of humor is Biograph's *How Charlie Lost the Heiress*, a 1903 film. He does so by graciously acceding to a Negro woman's request to mind her baby carriage for a moment. The heiress comes along, sees Charlie and the carriage containing black twins, expresses amazed horror, turns on her heel, and walks out of his life. Charlie, understanding what has happened, faints.[35] Similarly, in the 1908 Vitagraph comedy *Nellie the Beautiful Housemaid*, three elderly white men hire a domestic through the newspaper and go to great lengths before her arrival in order to make a good impression; the film ends when the door opens and an obese black woman enters the room. A trade magazine reviewer of one such film wrote in 1913 that "there have been more jokes perpetrated on the colored race than on any other" and added almost in surprise and obviously in ignorance of statements by such members of the black community as W. E. B. DuBois and Kelly Miller "yet our darker brethren never complain." The reviewer then went on to review as "the cutest little thing" the Pathe comedy *Mixed Colors*, its humor based on the aftermath of mischievous boys painting a black baby white and a white baby black and placing each in the other's carriage.[36]

But if a mixup between the races was considered the subject of comedy, a mixture of blood was the occasion for tragedy. In Peter Noble's pioneer work on the Negro in movies, the few pages on the black in American film prior to 1915, though spotted with errors, correctly maintain that in these films for a person portrayed

[34] *Moving Picture World*, Jan. 23, 1909, p. 93.

[35] *How Charlie Lost the Heiress*, print in the Library of Congress Paper Print Collection.

[36] *Moving Picture World*, Jan. 11, 1913, p. 166.

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as having mixed blood only death "provided a logical escape from a world in which to be partly colored was considered an even worse disgrace than to be a full-blooded Negro."[37] Perhaps no film emphasizes this more starkly than *In Humanity's Cause*, a 1911 production from the American branch of Eclair. A melodrama set in Civil War times, the film has as its main character a Confederate officer who is saved from death on the battlefield by a blood transfusion from a black man. After a title reading "Blood Will Tell," the Southerner is shown to be a changed man, in the words of the *Moving Picture World* reviewer "a brute who disgusts even his long suffering sweetheart."[38] Having found out about the transfusion, the transformed man hunts down the black man, finds him at the edge of a high cliff, grapples with him, and they fall to death locked in each other's arms. This film is an epitome of the whole good-white, evil-black equation with a bit of the *Doppelganger* theme thrown in. A more usual tale—and one which also includes the then generally prevalent assumption that if a mulatto has temporary success at anything, even villainy, it is due to the white blood in his veins—is the 1913 Rex film *In Slavery Days*. This drama begins with an octoroon who is responsible for the family baby in the parents' absence switching that baby and her own child; it ends with the white girl, grown up, reestablished in her rightful place and the death by fire of the octoroon and her daughter. For the mulatto happiness is possible only through a last-minute revelation that the character has no black blood, as in the 1914 Select Photoplay *At The Crossroads*, in which the heroine is able to marry her beau after all because her mother proves to be not an octoroon but a white of Spanish origin.

The Nigger, a 1915 Fox film based on Edward Sheldon's powerful but contrived play of the same name, deals with a newly elected, antiblack Southern governor who upon discovering that he is partly black feels compelled to give up his office and the white patrician woman he loves (even though she is willing to overlook his black blood). This grim film, which has matinee idol William Farnum in the title role, is noteworthy for the vigorous reaction it engendered in the black community. The brouhaha over *The Birth of a Nation*, released shortly afterward, has obscured the strenuous

[37] Peter Noble, *The Negro in Films* (London, n.d.), 29.
[38] *Moving Picture World*, Dec. 16, 1911, p. 905.

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but relatively ineffective campaign waged against *The Nigger*, a campaign that succeeded only in having Fox retitle the film *The New Governor* in some areas and in having bits of the more inflammatory scenes cut for showing in a few communities.[39]

A different reaction to the black's screen image had brought action two years earlier in Chicago. Starting in 1913, black showman William Foster, described by the *Chicago Defender* as "one of the best informed men in theatricals hereabouts," made a number of short films (mostly comedies) using all-black casts and dealing with what the newspaper called "real Negro life."[40] He believed that such an endeavor would be commercially viable. The Foster Photoplay Company, which had to rely on white financing because it could not get black support, seems to have been virtually a one-man operation (for example, Foster wrote, directed, and filmed some of the productions), and despite his well thought-out plans, the company soon foundered for lack of funds, technical know-how, and distribution outlets.

Discussing the American movie industry's treatment of the black prior to *The Birth of a Nation*, one scholar calls the various characterizations of the black presented to the public an "unformed image."[41] This judgment has some merit given the different stereotypes that came into use, even given the fact that they often overlapped. But it is also abundantly clear that the range of these stereotypes runs the gamut from *A* to *B* (to echo a famous stage criticism). Between the 1890s and 1915, the movie black—whether played by a white or not and whether presented as an uneasy menace, a dancing machine, a comic stooge, a faithful retainer, a cheerful flunky, a tainted unfortunate, or an ignorant savage—was shown in films as an amalgam of qualities that were the opposite

[39] Some indication of the campaign against *The Nigger* is evidenced in *Moving Picture World*, Apr. 17, 1915, p. 428; May 1, 1915, p. 760; May 29, 1915, p. 1481. In the June 19, 1915 issue, p. 1965, a report by a writer for the Kansas City News Service recounts the attempts by the Wichita black community to stop showings of the film and rather incredulously reports that "one negro minister wasn't sure but that it would have been all right for Georgia, the white girl, to marry Phil." See also, *The Chicago Defender*, May 15, 1914, p. 7.

[40] *Chicago Defender*, Aug. 9, 1913, p. 5; Sept. 27, 1913, p. 5.

[41] Thomas Cripps, "The Myth of the Southern Box Office" in James Custer and Lewis Gould, eds., *The Black Experience in America* (Austin, 1970), 118, n.2.

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of the values treasured by white American society. There was no upward social mobility for blacks in these films just as there was no way out of the stereotype prison for black actors.

Among social scientists and others concerned with the impact of movies on society there were and are strong divisions of opinion about whether a film influences an audience or reflects its ideas, but there is no argument about the fact that the movies played an important part in the American Dream. Almost from the film industry's beginnings in the United States the black on screen was left out of that dream, either by being ignored or by being presented as incapable of partaking by virtue of a not quite human nature. *The Birth of a Nation* is always cited as a milestone in film treatment of blacks. For some time prior to its release, however, just about everything that was and is traditionally held to be of some consequence in the United States was absent from the movie stereotype of the black. Patterns were set in these early motion pictures that with few exceptions would not be broken on screen for decades. John Oliver Killens has accused the American movie industry of being "the most anti-Negro influence in this nation" and has called *The Birth of a Nation* "Hollywood's first big gun in its war against the black American."[42] The castings for that gun were forged in the movies made between the 1890s and 1915.

[42] John O. Killens, "Hollywood in Black and White" in Schwartz and Disch, 401.

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The Consequences of Corruption: The Ghanaian Experience*

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Professor Arnold J. Heidenheimer, in his very useful collection of readings on political corruption, notes the tendency of American scholars to discount the dysfunctional impact of corruption both in the United States and abroad.[1] Henry Jones Ford, writing in 1904, saw political corruption in the United States as a means of overcoming the rigidities of the American constitutional system of checks and balances.[2] V. O. Key Jr. has argued that in the absence of a traditional governing class, American political parties had to rely on patronage (including the promise of jobs, contracts, and favorable legislation) to gain needed financial support and to direct or control their followers.[3] Moreover, Key points out that "bribery has been a convenient way to avoid legal requirements which may be impracticable of application," particu-

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[1] Arnold J. Heidenheimer, ed., *Political Corruption: Readings in Comparative Analysis* (New York, 1970), 185-86.

[2] Henry Jones Ford, "Municipal Corruption: A Comment on Lincoln Steffens," in *ibid.*, 284-93.

[3] V. O. Key Jr., "Techniques of Political Graft," in *ibid.*, 51-53.

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larly in circumstances of conflicting codes of moral conduct. Daniel Bell, writing more recently, emphasizes the use of corrupt and even violent practices by disadvantaged American ethnic groups to achieve upward mobility.[4]

Corruption, even when it was most blatant in nineteenth-century America, apparently did not retard economic development. Indeed, the epoch of the big city boss coincided with the period in American history of most rapid economic growth. This has suggested to some scholars that corruption actually facilitated economic development.

In view of this analysis of American experience, it is understandable that American scholars often minimize the extent to which corruption might retard the development of emerging nations. The assumption is that political parties in these nations act much in the manner of American political machines, motivated by individual self-interest and the possibilities of financial gain.[5] Regimes which lack an ideological or normative commitment have no alternative way of gaining support. Henry Bienen has written that political machines in Africa, as in America, benefit the poor and those ignorant of or opposed to the legal system.[6] The alien rich may play a useful economic role in Africa, and in the absence of formal political power, their survival may necessitate reliance on corruption. In any case, corruption may have the effect in emerging countries, as in America, of making the bureaucracy more humane and approachable, which is particularly important for those who have not yet adjusted to the modern industrialized way of life.

One American political scientist argues that corruption is functional in emerging countries because of the conditions under which legislation is generally formulated and administered.[7] Such countries usually lack the procedures for coordination, criticism,

[4] Daniel Bell, "Crime and Mobility among Italian-Americans," in *ibid.*, 160-64.

[5] Cf. J. David Greenstone, "Corruption and Self-Interest in Kampala and Nairobi," *Comparative Studies in Society and History*, VIII (1966), 199-210.

[6] Henry Bienen, "Political Parties and Political Machines in Africa," in Michael F. Lofchie, ed., *The State of the Nations: Constraints on Development in Independent Africa* (Berkeley, 1971), 203.

[7] Cf. Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, 1968), 69.

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and control that are necessary for meaningful legislation. Consequently, there is likely to be much confusion regarding legal requirements. Since the political process in these countries is seldom either open or competitive, the rules are generally considered to be biased in favor of certain dominant groups or unrepresentative of public opinion. For these reasons, legislation tends to be poorly formulated in emerging countries, and corruption can serve to reduce the impact of the mistakes that are made. Moreover, administrators in developing areas tend to be improperly chosen, supervised, promoted, and paid. Thus, they may be not only incompetent, but also unable to command the respect of others. Corruption can act as a "lubricant" to overcome excessive bureaucratic inflexibility, sluggishness, and bungling.

Without denying these functions of corruption, one can suggest that political corruption, defined as the diversion of public resources to nonpublic purposes, might nevertheless be a greater cause for concern in emerging nations than in the United States or some other so called developed country.[8] It is in this regard that the Ghanaian experience is enlightening. First of all, Ghana probably has conducted and published more official investigations of corruption than any of the other third world countries. While most of these were undertaken in 1966 and 1967 to discredit the Nkrumah regime, they quite carefully followed British investigatory commission procedures. In addition, there is an abundance of useful writing by Ghanaians and foreigners on corruption in Ghana.

The Consequences of Ghanaian Corruption

Nkrumah left Ghana with a serious balance-of-payments problem. Beginning with a substantial foreign reserve fund of over $500 million at the time of independence, Ghana, by 1966, had a public external debt of over $800 million. This meant that there was no foreign exchange to buy the spare parts and raw materials required for the economy. While inflation was rampant, causing

[8] This definition of political corruption was suggested to me by Professor Victor T. Le Vine. However, no definition of corruption can be completely satisfactory for reasons suggested by me in an article on the causes of corruption in Ghana to appear in a forthcoming issue of the *Journal of Modern African Studies*.

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the price-level to rise by 30 per cent in 1964-65, unemployment was also serious. The needs and demands of the population under these circumstances simply could not be met.

Whereas between 1955 and 1962 Ghana's GNP increased at an average annual rate of nearly 5 per cent, there was practically no growth at all by 1965, despite the increase of central government expenditure by 78 per cent between 1960 and 1965 at constant prices. Since Ghana's estimated annual rate of population growth was 2.6 per cent, her economy was obviously retrogressing.[9] While personal per capita consumption declined by some 15 per cent between 1960 and 1966, the real wage income of the minimum wage earner declined by some 45 per cent during this period.

How much of Ghana's economic troubles can be attributed to corruption is difficult to determine. The decline in the world price of cocoa, from which two-thirds of Ghana's export earnings were derived, was certainly an important factor. But irresponsible external borrowing, amounting to almost $500 million between 1961 and 1966, together with the mismanagement of resources, must be considered equally responsible for what happened. The Ollennu Commission report reveals that instead of carefully controlling imports, the processing and issuing of licenses was "so manoeuvred as to lead to complete economic chaos and bankruptcy."[10] In some cases, much bigger licenses than desired were granted in order to increase the size of the kickback involved. Likewise, the size of the commission rather than the usefulness of the purchases was the determining factor in many governmental transactions.

Under Nkrumah, more than 40 state corporations were set up, involving an investment of over £40 million. Most of these proved to be a great drain on the economy, particularly the State Mining Corporation, Ghana Airways, and the State Farm Corporation. In the 1963-64 financial year, over one-third of the invested capital

[9] Cf. Justice G. C. Mills-Odoi, chairman, *Report of the Commission on the Structure and Remuneration of the Public Services in Ghana* (Accra-Tema, 1967), 27; Barbara Callaway and Emily Card, "Political Constraints on Economic Development in Ghana," in Lofchie, 65-92; Jon Kraus, "Arms and Politics in Ghana," in Claude E. Welch, ed., *Soldier and State in Africa* (Evanston, 1970), 178.

[10] *White Paper on the Report of the Commission of Enquiry into Alleged Irregularities and Malpractices in Connection with the Grant of Import Licenses* (Accra-Tema, 1966), 2.

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was lost, and in 1966, a loss of over $100 million (N₵ 89.4 million) was incurred by 22 state corporations.[11] While many state enterprises were unrealistic to begin with, such as a steel mill for which there was neither adequate domestic raw materials nor trained personnel, they also suffered from corruption.

In the case of the State Furniture and Joinery Corporation, the relationship of corruption to the development of a debt of nearly $350,000 between the end of 1961 and the beginning of 1968 has been clearly revealed.[12] During these years furniture was bought from private contractors contrary to the corporation's official objectives and without following official tendering procedures. Raw materials and machinery were also bought in an irresponsible way, often at highly inflated prices. At the same time, the corporation's raw materials, such as timber, lacquer, and thinner, and its machinery were sold on credit to certain favored private furniture manufacturers or practically were given away free of charge without proper advice, approval, or record keeping. Similarly, customers, particularly politicians from the Convention People's party, were given credits and discounts for furniture without any assignable cause or reason and without authority. Many refused to pay what they did owe, and no serious effort was made to collect debts. There were also irregularities in the payment of staff salaries, the keeping of accounts, and the handling of corporation funds. In these and other ways, "the assets of the Corporation were drained into the sea."[13]

Ghanaians are aware that corruption is a universal phenomenon. Yet they argue that it is a more serious matter in a poor country than in a country like the United States where illegally gained money goes back into internal production.[14] The profits from corruption in Ghana tend to be invested or banked abroad or spent on

[11] Cf. *The Ghanaian Times*, Dec. 4, 1969; K. A. Busia, *Africa in Search of Democracy* (London, 1967), 81. Trying to translate cedis into dollars is difficult both because of the changing "real" rate of exchange as well as the changing official rate.

[12] Cf. S. A. X. Tsegah, chairman, *Report of the Committee of Enquiry into the State Furniture and Joinery Corporation* (Accra-Tema, 1967).

[13] *Ibid.*, 23. In addition, certain firms were allowed to use the corporation's import licenses to order goods for their own benefit, thereby avoiding not only import controls but also certain taxes.

[14] Cf. J. A. Peasah, "Institutionalized Corruption," *The Legon Observer*, Feb. 17, 1967, pp. 12-13.

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wasteful imports. Moreover, the resulting conspicuous consumption is more demoralizing in countries where heavy public sacrifice is required for the sake of economic progress. Since many of the levy collectors in rural areas managed to own houses and farms while supposedly earning just a few cedis a month, it is understandable why "many a farmer refuses to pay his levy because of what he sees in his village."[15] Similarly, in Accra, many ratepayers refused to pay what they owed because they did not see any benefits to be derived.[16] How could the government admonish people to tighten their belts, Victor Owusu bravely asked during the Nkrumah period, when "members of the new Ghanaian aristocracy and their hangers-on, who tell them to do this, are fast developing pot bellies and paunches and their wives and sweethearts double chins in direct proportion to the rate at which people tighten their belts?"[17]

Corruption intensifies interethnic conflict in Ghana. "You have only to scratch an African and you see tribe," observes one writer, suggesting that Ghanaians are more ethnocentric than they had heretofore believed.[18] Although interethnic tension is a problem faced by almost every country, it takes on added dimension when competition for jobs is extreme, procedures for resolving disputes are inadequate, and institutions for ameliorating discontent are weak. Consequently, corruption often takes the form of ethnic discrimination, leading to still more corruption.

As Nkrumah, for example, became more insecure, he became more concerned with the tribal origins of civil servants and high party functionaries.[19] He therefore increasingly surrounded himself with Nzimas (his own tribe), despite the fear of enlightened Nzimas that to do so would arouse the animosity of the other tribes. The leaders following Nkrumah have continued his practice of putting fellow tribesmen into strategic positions of power and authority "in order to settle old scores and/or buttress their

[15] Y. D. Agyeman-Dickson, "Local Councils," *The Ghanaian Times*, Nov. 19, 1969.

[16] E. A. Kwapong, "Accra City Administration," *The Legon Observer*, Dec. 9, 1966.

[17] Quoted in T. Peter Omari, *Kwame Nkrumah: The Anatomy of an African Dictatorship* (Accra, 1970), 90.

[18] Morre Bossman, "Tribalism in Ghana," *The Legon Observer*, July 21, 1967, p. 21.

[19] Omari, 96.

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own positions in the top-most institutions of the state. . . ."[20] The struggle for political power thereby becomes a struggle for tribal advantage. Those who lose must face not merely a loss of the perquisites of political power but also, in so far as there are insufficient restraints on the use of political power, tribal discrimination in all aspects of life.

In Ghana, the public sector spends a far larger proportion of the national income than is the case in Western countries. By the end of 1964, nearly 70 per cent of Ghanaian wage earners were public employees, who were then responsible for nearly all of the significant sectors of the economy and for 27 per cent of manufacturing output.[21] In the United States, on the other hand, most essential services, including electricity, communication, and transportation, are furnished by private firms. As bad as corruption may be in an American city, basic services, if profitable, continue to be performed. While profitmaking incentives may induce corruption, they also facilitate the performance of work. Profits may be excessive, but there is at least a certain output. Internal corruption may be a problem, but it is seldom allowed to destroy the firm. Moreover, even the failure of a private firm is unlikely to have the same social impact as the failure of a public agency.

Since so much depends on the public sector in Ghana, the consequences of political corruption, combined with mismanagement, are correspondingly great. In Accra, for example, this combination has had a devastating effect on municipal administration,[22] resulting in the demise of the public transportation system. Prior to 1958, the Accra-Tema City Council's Omnibus Service was reasonably satisfactory and profitable. Between 1958 and 1964 the bus service was taken over by the Municipal Transport Authority, but it ceased to be either profitable or efficient because of the "shortage of spare parts, difficulties in obtaining import licenses, undue political influence, embezzlement and stealing and employment of redundant staff."[23] The city council was allowed to resume operation of the bus service in 1965 but with only 28 functioning

[20] Publius Gamesu, *The Legon Observer*, May 22, 1970, p. 2.

[21] Cf. Henry Bretton, *The Rise and Fall of Kwame Nkrumah* (New York, 1966), 106; Callaway and Card, 81.

[22] Cf. E. K. Akyea-Djamson, chairman, *Interim and Final Reports of the Commission of Enquiry into the Accra-Tema City Council* (Accra-Tema, 1969).

[23] *Ibid.*, 145.

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buses, instead of the 123 that it had had in 1958, and with only a small percentage of its former assets. In 1965, 120 American buses were imported, costing almost 50 per cent more than the British buses which the city had been using. This purchase was initiated by the executive-chairman and the town clerk, allegedly under pressure from Nkrumah, with the standard 10 per cent kickback in mind, but without council approval and without independent technical or legal advice. These buses proved completely unsuitable for Ghanaian urban conditions. Only 8 of the first 50 were functioning less than two years after they were put into operation. The resulting lack of a municipal bus service makes late arrival at work in Accra understandable and excusable, but hardly conducive to national productivity.[24]

In many of the smaller Ghanaian communities, the abuses of political power have had an even more deleterious effect than in Accra. The commercial life of Swedru was badly disrupted in 1962 when the government took over the European trading firms and established the Ghana National Trading Company (GNTC).[25] The small traders and shopkeepers could no longer get the advances of goods and credit which they depended upon. Only loyal party members and friends of the local district commissioner could hope to receive desired goods which were in short supply because of import restrictions. These traders could then resell these goods often at several times their original worth to customers who would retail them at even more exorbitant prices to the general public.

The manifestations of corruption in Ghana have perhaps been most apparent in relations between and within governmental units. Ideally, these relations should be characterized by what I have elsewhere referred to as "elasticity of control."[26] This term suggests a cooperative relationship, which is mutually gratifying rather than disruptive, in which political power takes more of a

[24] Whereas the official workday is 7:30-12:30 and 2:00-4:30, the "most convenient" time to conduct government business, foreigners are advised, is 9:00-10:30 and 3:00-4:00.

[25] Maxwell Owusu, *Uses and Abuses of Political Power* (Chicago, 1970), 271-73.

[26] Cf. Herbert H. Werlin, "Elasticity of Control: An Analysis of Decentralization," *Journal of Comparative Administration*, II (1970), 185-209; Werlin, "The Nairobi City Council: A Study in Comparative Local Government," *Comparative Studies in Society and History*, VIII (1966), 183-86.

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persuasive than a coercive form. Under these circumstances, interference of a disciplinary or compulsory sort is potentially possible but actually rare, while varying forms and degrees of supervision and guidance are common.

In underdeveloped countries, the situation is relatively "inelastic." The exercise of political power does not readily increase or decrease in reaction to need. It does not flow as a gently fluctuating stream of influential suggestions. This inelasticity results from a general lack of cooperation, so that control tends to be authoritarian or ineffectual, or a sporadic alternation between the two poles. When demand is responsive to changing price, economists speak of "elasticity of demand." Likewise, when political power is responsive to forms of control, we can speak of "elasticity of control." But where those to whom power has been granted do not respect those who are supposed to control them, we must consider that control to be inelastic. Corruption contributes to inelasticity of control in so far as it reduces the capacity of those in authority to guide subordinates.

The effect of corruption is to generate an atmosphere of distrust which pervades all levels of administration. This discourages top leaders or administrators from preparing subordinates to undertake an increasing amount of the requisite workload and decisionmaking. At the same time, their capacity to supervise and correct what goes on is also reduced. Consequently, the delegation of authority which is essential for successful administration becomes impossible. Yet where decentralization cannot take legitimate institutional forms, it is likely to take various unauthorized forms. This accounts for the common situation of underdeveloped regimes appearing to be authoritarian but actually being impotent.

A number of the Ghanaian commission reports illustrate the point I am making here. For example, many of the problems of the State Furniture and Joinery Corporation stemmed from the appointment of a managing director with nothing more than political qualifications. Even the minister responsible for the affairs of the corporation during the Nkrumah period described the managing director as "a person without technical know-how . . . who spent most of his time outside his office doing nothing, much to the detriment of the Corporation."[27] Because the branch managers

[27] Tsegah Committee Report, 5.

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did not respect him, they refused to carry out his instructions and to give him the technical information he needed. In the words of the Tsegah Committee report, the whole corporation was reduced to a "leaderless army": "Every employee of the Corporation took advantage of the situation and did what he or she liked. The Management failed to control the workers with the consequence that production dropped miserably."[28]

While Nkrumah remained scornful of the postcoup commission reports, he admitted that his own efforts to reduce corruption were of limited success: "My difficulty was to get the police to enforce the principles I laid down."[29] Henry Bretton asserts that Nkrumah was never serious about his periodic campaigns to reduce corruption: "It was not a surprise to his Commissioner of Police when Nkrumah, upon being informed of spreading bribery and corruption in the highest government and party circles, time and again turned down requests for punitive, corrective action."[30] Nkrumah's anticorruption campaigns, Omari suggests, were not so much to reduce corruption as to put fear into his underlings, thereby improving his own tactical position.[31]

Geoffrey Bing, the Englishman who served for many years as Nkrumah's attorney-general and chief adviser, acknowledges Nkrumah's failure to follow through in any meaningful way his denunciations of corruption, particularly his famous Dawn Broadcast of April 8, 1961. Bing, however, places the blame not on Nkrumah but on the civil service which, "from the top post downwards, were opposed to any showdown which might imperil the existing structure of power."[32] This observation is confirmed by a former organization and methods officer in the Nkrumah government, who points out that the recommendations of the Organization and Methods Unit set up in 1961 tended to be suppressed or ignored "because of the political influence which could be wielded by the people directly affected by those recommendations."[33] It is confirmed also by Owusu, who notes that "it is often said in Gha-

[28] *Ibid.*, 58.

[29] Kwame Nkrumah, *Dark Days in Ghana* (New York, 1968), 69.

[30] Bretton, 186.

[31] Omari, 146.

[32] Geoffrey Bing, *Reap the Whirlwind: An Account of Kwame Nkrumah's Ghana from 1950 to 1966* (London, 1968), 408.

[33] M. A. Bentil, "The Development of the Organization and Methods Service in Ghana," *O & M Bulletin*, No. 9 (1968), 27.

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na that the president should be sympathized with, since he had no effective way of controlling the atrocious behavior of his ministers, R. C.'s [regional commissioners], and D. C.'s [district commissioners]."[34] But nothing else could have been expected, Omari argues, because "when political men sell their souls for worldly goods, it takes a greater god than Kwame Nkrumah to inspire them to disinterested deeds in the service of their country."[35]

Nkrumah's obsession with having "yes men" around him did not indicate that he trusted them. Even the police and the Special Branch, which were supposed to have primary responsibility for security, could not be trusted according to Nkrumah, because they "have been involved in each of the six attacks made on my life, and have frequently ignored, and sometimes aided, the activities of people they knew were plotting to overthrow the government."[36] Yet, his subsequent effort to foster security by utilizing rivalries within the government, by establishing competing spy networks and by employing foreigners in key positions gave rise to the subversive movement he most feared.

Nkrumah's increasing paranoia had the effect of stifling innovation. "As a result," to quote Henry Bretton, "the learning and correcting capacity of the government and administration of Ghana was submerged in a welter of irrational, contradictory, erratic, highly emotional perspectives concerning events at home and abroad: The learning capacity of Ghana was reduced to the learning capacity of Kwame Nkrumah."[37] Similarly, the widespread corruption and the ineptitude of local governments and field agencies brought about an excessive centralization of authority in the ministries in Accra. This meant that "even minor decisions are passed up a long hierarchical line to be taken by some anonymous and unaccountable authority far removed from the job or the local consequences."[38] The ministries, therefore, tended to neglect their primary responsibility for determining objectives, priorities, and strategies for the nation as a whole, and for assessing, marshaling, and allocating resources. Yet, they could never be certain of having their instructions followed at the local level.

[34] Owusu, 321.
[35] Omari, 5.
[36] Nkrumah, *Dark Days*, 41.
[37] Bretton, 142.
[38] Mills-Odoi Commission Report, 2.

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Ultimately, the effect of corruption in Ghana was to undermine the legitimacy of the regime. After all, the purpose of independence, according to Nkrumah, was to create a better life for all, in accord with the philosophy of African socialism. This goal implied an emphasis on equality, cooperation, collective welfare, and sacrifice. It was supposed to inspire enthusiasm, community action, and voluntary labor: "The socialist pattern of society which we envisage for Ghana can only be constructed upon a high level of productivity, of education, of civil responsibility and a spirit of enterprise and service."[39] Corruption, it could be argued, might facilitate the private profitmaking appropriate in a capitalist system, but it could never be reconciled with the maximization of public welfare promised by socialism. The existence of corruption mocked Nkrumah's advocacy of socialism; it undermined personal integrity and substituted personal gain for an ideological commitment as the basis for loyalty to the political system.

Conclusion

This essay began by noting the tendency of American scholars to belittle the concern in emerging nations with the problem of corruption. Some go so far as to suggest that these nations could benefit from emulating the techniques of American political machines for reconciling diverse interests and for responding to new economic and social needs.[40] In other words, they consider the irrational or irresponsible use of corruption, rather than corruption *per se*, to be a barrier to economic progress and political stability. This analysis, however, appears to rest on a misunderstanding of the American experience. American development could take place, despite corruption, because of certain offsetting factors. First of all, the power of American political machines in their heyday rested on the legitimacy of the American political system. They were respected because they could organize the electorate in such a way as to win elections. This function was necessary because the political system remained highly competitive, even when dominated in certain areas by a particular party or faction. The political ma-

[39] Kwame Nkrumah, *I Speak of Freedom* (New York, 1961), 92.

[40] Cf. James C. Scott, *Comparative Political Corruption* (Englewood Cliffs, N. J., 1972), 114. This book contains a very useful analysis of corruption.

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chine was forced to ingratiate itself with the public in such a way as to withstand persistent challenges. Likewise, the political machine could selectively enforce laws or collect taxes for the benefit of supporters because of the certainty that laws could be enforced and taxes could be collected. Here, too, popular consensus was necessary; but the maintenance of this consensus obliged the political machine to follow the rules of the governmental process. This meant that a local boss had to remain legally accountable for his actions to the elites at higher levels who determined, executed, and adjudicated these rules. He could certainly misuse his position and the other elected or appointed positions which he controlled, but he had to do so in a manner calculated not to unduly offend neighboring and higher levels of government or a press that might be anxious to embarrass him.

The patronage and wealth available to an American political machine may have been immense, especially in metropolitan areas, but these depended on the vitality of the American society and economy. While the American political machine could exploit a wide variety of business, occupational, and social groups, it had to respect and cater to these groups. Anything which undermined these groups or caused them to leave the community reduced the capacity of the political machine to furnish the jobs, contracts, and favors which made it popular. Yet, unless the jobs, contracts, and favors were given in such a way as to maintain the quality of public services (including education, police protection, roads, sanitation), the machine was in danger of alienating those upon whom it depended. For example, minor violations of a building code—particularly a badly outdated one—might be overlooked for the benefit of a friendly contractor, but certainly not if these violations were likely to result in the collapse of a series of buildings. This would mean facing the hostility of those with formidable financial, legal, and communication resources. These same resources could also be used to prevent rivals from gaining excessive advantage through corruption.

For American groups lacking adequate means for defending themselves, such as Indians, Mexican-Americans, and blacks, however, the impact of corruption has been absolutely devastating. It is well known that in their communities crimes often go unpunished, streets are left unpaved, schools are allowed to deteriorate,

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and other public services are neglected. At the same time, merchants, moneylenders, and landlords are left relatively free to take advantage of the situation. But when people are poor, ignorant, discouraged from political participation, and lacking informal sources of leverage or contact, what means do they have for redressing grievances and resisting exploitation? They can only react by furtively or violently attacking the public and private establishments around them or by retreating into alcoholism, drug addiction, and apathy; but this only intensifies the lawlessness and malaise from which they suffer, reducing still further the services and opportunities available to them.

In Ghana, as in other emerging nations, the factors that could soften the destructive effects of corruption are also inadequate. There are no political parties, independent newspapers, courts of law, competing levels of government, or functional associations that can be relied upon for protection. Moreover, even if there were, few people have the education, information, and status necessary to utilize them effectively. Traditional associations might serve a supportive function, but they too have their limitations, giving rise to possibly disruptive interethnic conflict. The latest military intervention is apparently a source of consolation to some Ghanaians, but this can hardly change the system.

What makes the situation so hopeless in a country like Ghana is that the very factors which encourage corruption prevent any major reform from taking place. By undermining the constitutional restraints limiting governmental action, Nkrumah and his successors vitiated the legitimacy of their rule. Men obeyed them because of coercion or financial inducements, not because of any deep respect for their position or commitment to the objectives of their regimes. But these penalties and rewards did not encourage improved performance of public service. Indeed, constructive innovation would have entailed certain kinds of sacrifice which would have made the spoils of office less attractive, thereby reducing the value of what was offered. It would have also entailed hiring and promoting people on the basis of ability rather than subservience.

K. A. Busia, in his book, quotes Lord Acton's famous aphorism, "Power tends to corrupt, and absolute power tends to corrupt absolutely."[41] However, what he must have discovered in his period

[41] Busia, 98.

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as Ghana's prime minister was that corruption stems also from the absence of power. Busia, as much as Nkrumah, relied on patronage to ensure loyalty, but this did not give him the control he needed to reduce corruption.

Busia, like Nkrumah, attempted to prevent prices from reacting to the economic imperatives of supply and demand through measures such as rent control, import licenses, rationing, and foreign exchange control. Even highly developed bureaucracies, however, find such measures difficult to enforce. Consequently, the greater Busia's efforts to implement these measures, the more they were undermined by corruption, the situation forcing him eventually to drastically revalue Ghanaian currency. This unpopular move, more than anything else, led to his downfall.

Also, like Nkrumah, Busia found, as his economic problems increased, that the financial inducements at his disposal were inadequate to satisfy his subordinates as well as the general public. "It is predictable," writes Maxwell Owusu, "that any political structure in Ghana will, in the short run, remain highly unstable until the ideal of individual and group economic sacrifice . . . and of hard work has been generalized and internalized or accepted as normal by a significant section of the national population, particularly the leadership."[42] Until this happens, we can add, there can be no escape from the havoc of corruption.

[42] Owusu, 322.

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Revolution and Growth: Two Case Studies from India

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One of the most severe contradictions radical movements face is the contradiction between the drive toward radical transformation of their societies and the need to maintain production in the largely capitalistic economy during the period of transition and change. The conflicting demands imposed by this contradiction often lead regimes committed to change to abandon their plans in order to avoid antagonizing their business communities—to avoid a "strike of capital." Other regimes are forced in the face of such a strike of capital to nationalize the various branches of the national economy in hopes of keeping them going. Unprepared to undertake the task of economic management, the state apparatus often falters. The resulting crisis of production becomes a crisis for the regime, which often falls or resorts to repression to compensate for its declining popular support.

The difficulties of the government are often rendered more manageable if it can ally itself with some important sectors of the economy while opposing others—say with domestic, as opposed to foreign, capital or small, as opposed to large, producers. The second stage of the New Deal when it adopted a Brandeisian antitrust policy was an attempt to ally with small business, as contrasted with an earlier attempt to ally with all of the business

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community during the time of the NRA. One can speculate that Roosevelt turned on the "economic royalists" in 1935-36 because he feared that the reversal in the road to recovery at the time was the result of a capital strike.

Nation-states, in any case, have considerable leeway in dealing with their capitals. They often have controls of greater or lesser efficiency to prevent the flight of capital. In addition, that portion which is employed by entrepreneurs on their own account is often especially immobile because of the disinclination of entrepreneurs to leave their native lands.

Within federal states the constituent units are under more severe constraints, because movement of capital is often, both formally and informally, easier to achieve. The Southern states of the United States for the last eighty years have tried to lure capital by suppressing trade unions, offering low tax rates, and otherwise making concessions to capitalists. The relative failure of this effort suggests that other factors in the U.S. have been far more important in determining the location of new investment. In fact, there are indications that many firms preferred other locations, such as California, because of the higher standard of public services available there to their employees. Still, despite higher taxes, the potential for interstate competition undoubtedly leads to restraint on costly social experimentation on the part of states and municipalities.

Two of India's seventeen states, West Bengal and Kerala, have experienced government by Left-Communist coalitions over the last few years (Kerala: 1957-59 and 1967-69; West Bengal: 1967 and 1969).[1] Both regimes were dominated by the CPM (Communist Party-Marxist) an independent party originally sympathetic to China on the Sino-Soviet split, though now roughly neutral—or, rather, as Marcus Franda suggests in a recent book, attempting

[1] Marcus Franda, *Radical Politics in West Bengal* (Cambridge, Mass., 1971). Myron Weiner, *Political Change in South Asia* (Calcutta, 1963). Sankar Ghosh, *The Disinherited State* (Bombay, 1971). K. G. Krishna Murthy and G. Lakshumana Rao, *Political Preference in Kerala* (New Delhi, 1968), gives a formal analysis. Stephen Oren, "Religious Groups as Political Organizations: A Comparative Analysis of Three Indian States" (Ph.D. diss., Columbia University, 1969). C. Achutha Menon, *What Happened in Kerala: Review of the 30 Months of the Namboodiripad Government* (New Delhi, 1969). E. M. S. Namboodiripad, *Kerala: Yesterday, Today, and Tomorrow* (Calcutta, 1968).

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to paper over divisions among its own membership on the split.[2] It is differentiated from the pro-Moscow Communist Party of India (CPI) on its right by its implacable opposition to Indira Gandhi.[3] It is differentiated from various small groups on its left—all called Naxalites, from Naxalbari, the location of a famous peasant rising in 1967—since it is willing to make tactical and perhaps strategic use of electoral politics.[4]

Kerala and West Bengal have many features in common. They are poor, though Kerala is by far the poorer. They have cultures sharply different from those of neighboring areas. There is widespread literacy and higher eduction, though Kerala is far ahead in both respects.

But the differences between the two states are more impressive and it is these differences that partially explain the different impact of CPM rule in the two states and the persistence of the CPM as a large party in both. Roughly 20 per cent of India's industry was located in West Bengal at Independence in 1947—considerably less now—and 10 per cent of India's population resides there. Kerala, only a little smaller, has a much smaller proportion of the country's industry, and even with its vigorous development over the last few years is still one of India's least industrial states. While more than two million immigrants from western India have moved to West Bengal as industrial workers, large numbers of Keralans have moved all over India in search of employment.

The "panic" of capital which followed the first United Front government in West Bengal in 1967 led to a virtual cessation of new investment. Almost no applications for private investment licenses were made in West Bengal between 1967 and 1971. This is partially because of the insecurity businessmen felt during the violent factionalism of the last few years (several thousand people, mostly political activists, have been killed). But the cessation of new investment can be more directly traced to the rapid increase in strikes in the years 1967-70, characterized by bitter interunion rivalry. These strikes seem to have had two results. Many firms with plants only in Bengal lost much of their market share during

[2] Franda, 182-214; Mohan Ram, *Indian Communism—Split Within a Split* (New Delhi, 1969).

[3] Franda, 215-41.

[4] Mohan Ram, *Maoism in India* (New Delhi, 1971).

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the prolonged work stoppages, and have responded by seeking to locate new plants elsewhere, to prevent a recurrence of that loss.

The second result has been a rapid rise in factory wages combined with the acceptance of productivity-lowering work practices, so that one firm, for example, that had been clearing 15 per cent on its invested capital now clears 6-8 per cent. While the new rate is still profitable enough that they and others like them, unlike Brooklyn landlords, are not eager to abandon their assets, it is not high enough to encourage new investment. (The central government, in any case, restricts the transfer of existing plants, but cannot exert as effective control over new investment).

The movement of new investment elsewhere has been further facilitated by the fact that most of West Bengal's industrial capital is controlled by firms that already have large interests in other parts of India and the world. A decision to invest elsewhere does not mean a venture into terra incognita. The fact that firms without such branches elsewhere, especially those owned by Bengalis themselves, have been the first to move out suggests that the results of the wave of strikes, rather than the footlooseness of West Bengal's capital, was indeed decisive.

Kerala also experienced some trade union militancy, but none of the bloodletting that occurred in West Bengal. More importantly, productivity seems to have been maintained. A considerable industrial expansion continues. On the one side, the vigorous indigenous small entrepreneurial class continues to expand. On the other side, several bureaucratic entrepreneurs are responsible for a rapidly expanding industrial group owned by the state itself. A fair number of factories owned by the central government, such as the new refinery in Cochin, have been located in the state.

West Bengal has received one large steel mill in Durgapur and is scheduled for a Rs. 550 million oil refinery at the port of Haldia, in the central government sector. But with West Bengal's much larger urban and industrial population the impact will continue to be minimal.[5]

Kerala has continued to receive, in addition, funds from non-indigenous large-scale businessmen, sometimes solicited by its CPM leadership. Their success is perhaps related to another differ-

[5] Sarwar Lateef, "West Bengal's Economy—No Simple Recipes for Survival," *Statesman*, Aug. 18, 1971, p. 8.

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ence between the states.

The CPM in Bengal contains, as Marcus Franda reports, two balanced tendencies—one interested in electoral politics; the other, with its origin in the terrorist movement, oriented toward a variety of political modes. There appears to be a similar but less sharply drawn tension in the Kerala CPM, but the "electorals" are so clearly dominant there that the tension does not hamper the party's action as it does in Bengal. The greater freedom of action and ability to coordinate the actions of different wings of the party may increase the party's ability, when ruling, to conclude balanced agreements with the managers of capital.

This difference is closely related to the character of the constituencies of the CPM in the two states. The CPM of Kerala by its successful organization of agricultural laborers unions and militant participation in land seizures has consolidated itself as the party of Kerala's poor and landless—though like any other party it attracts support and especially leadership cadres from better off groups as well. A much larger percentage of CPM support in West Bengal derives from the middle class. Every study before 1967 showed that the CPM won a much higher vote in middle-class constituencies than among factory workers. There are some indications in the 1971 state polls that segments of the Bengali middle class have turned from the CPM and have been replaced by industrial workers and agricultural laborers. The diversity within the membership may explain the greater factionalism in the West Bengal cadres, since there seems to be a much greater propensity toward factionalism in radical parties recruited from middle-class social bases than from those whose base of support and cadres come mostly from the poorer classes.

A final set of factors to be considered is related neither to the nature of the CPM nor to the level of industrialization. West Bengal has been a state in decline over the last twenty years, some would say the last fifty years. Its agricultural productivity is low and stagnant, which has not been the case, in general, in Kerala, except for the recent decline in high capital intensity land reclamation efforts because of the insecurity of tenures.

Government efforts have not been very effective in improving Bengal agriculture. The failure may not be solely the fault of her administrators, because the same problem afflicts East Bengal (now Bangla Desh) and the neighboring Indian states of Bihar,

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Assam, and Orissa. There is some hope now that a new high-yielding seed rice technology has begun to catch on, but official published figures do not as yet show much progress. The lack of a progressive agriculture has removed one potential stimulus to industrial growth. By way of contrast, many new branches of Keralan industry, such as shrimp processing, are connected with the development of her food-producing industries.

The industries of West Bengal's premier city, Calcutta—jute and engineering contracting for the railroads—are in decline.[6] The decline of the jute industry is a result both of the development of substitutes for the former main product of the industry, burlap sacking, and of the creation of a competing industry in East Bengal, formerly the source of the raw materials. The railroad workshops were outmoded and new branches of engineering slow in developing. Perhaps with the independence of Bangla Desh, the government will start locating more defense industries in Calcutta, now less exposed to attack. But besides further development by the public sector around the petroleum nucleus at Haldia and perhaps the large steel plant at Durgapur, it is not clear what the source of new industrial investments for the state will be. The violence and strike waves have abated, and there is real potential for reform in the system of industrial relations to make their recurrence less likely. But high wages and low productivity remain, and new private investors have not stepped forward.

Lastly, the decline of Calcutta's staple industries have been accompanied by a deterioration of her infrastructure, under the weight of neglect and a rocketing refugee population, since 1950. The matter has finally been taken in hand, and large amounts of central government funds have been committed to refurbish Calcutta's systems for transportation, water supply, refuse disposal and drainage, and housing.[7] More important for the skeptical observer of the previous years, work has actually commenced on

[6] Hirenmae Karlekar, "West Bengal—The Tasks Ahead," *Statesman*, Mar. 4, 5, and 7, 1971, pp. 6, 8, surveys the industrial and infrastructural problems. A textbook is M. R. Chaudhuri, *The Industrial Landscape of West Bengal: An Economic-Geographic Appraisal* (Calcutta, 1971).

[7] Sponsored by the Bengal Chamber of Commerce and Industry in Calcutta, a study entitled *West Bengal: An Analytical Study* (New Delhi, 1971) summarizes much of the work done in the 1950-60 period especially by the Calcutta Metropolitan Planning Organization. The publications of the Calcutta Metropolitan Planning Organization, itself, are also available. See the *Statesman* of Aug. 26, 1971, p. 7, for details of new public works.

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large-scale road repair (11,000 roads) and *bustee* (shanty town) improvement. A large addition to the city's overloaded water plant has been commissioned and plans made for water sources and refuse-disposal channels in many of the shanty towns, in which one-tenth to one-fifth of the city's population lives.[8]

These two cases suggest that the impact of a strong regional radical movement within a federal state will vary according to circumstances. The existence of a large complex industrial base makes it more necessary to come to some accommodation with the private sector—to avoid economic breakdown. A strategy of straight confiscation is usually barred by the central authority, even if the newly established radical regime were ready to take over the administration of the state's industrial plant. An understanding with a segment of the business class is always desirable, and it is more easy to effect if the radical movement is not fragmented. Paradoxically, the lack of fragmentation, and thus tactical compromise, may be easier to achieve if the radical movement's constituency is poor rather than middle class. Lenin's famous dictum about trade unionism as the natural position of proletariats may not accurately predict the direction events will take. It does help explain the relative conservatism of the organized segments of the industrial working force in upheavals as widely differing as the May Days of France and the Cuban Revolution. In West Bengal, however, it was precisely the trade-union situation that undermined the possibility of rapprochement with private industry.

These cases indicate that deeper facts of environment and history rather than the details of specific regimes may be the prime determinants of new investment in federated units.

We should notice that both CPM-led regimes were toppled in 1969. In the 1971 elections they both increased their own percentage of the votes and seem to have consolidated for themselves the support of one-third of the electorate. At the same time, an anti-CPM consensus has emerged, which seems to promise that the CPM will remain a perpetual minority. The regimes that are likely to prevail will probably find it difficult to undertake reforms, as in similar situations in France and Italy, for fear of losing some of their support. In the Bengal case, the role assumed by small-

[8] See the *Statesman* of Sept. 1, 1971, p. 1 for details on new waterworks.

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scale Bengali businessmen in the secondary leadership cadre of the Congress suggests a new alliance in which these businessmen may take some responsibility for initiating new enterprise. It would seem that the CPM's fall from power is likely to make the two CPM movements less amenable to compromise—and in the Bengal case, it is hard to see much hope for a private economic revival without some such compromise with the forces the CPM now represents. The central government appears unwilling to invest enough to arrest the state's industrial decline.

Some entertain hopes of commerce with the newly independent Bangla Desh. But twenty years of separate development have helped render it economically competitive rather than complementary to West Bengal. The poverty and stagnation of Bangla Desh make it unlikely that it will give much stimulus to West Bengal's economy. The values of Bangla Desh autonomy are more likely to be seen in cultural and intellectual matters than in the steel ingot and jute bag industries.

Both the cases of West Bengal and Kerala indicate the limits on revolutionary change—as contrasted with revolutionary expression—in units of a federation. Perhaps the CPM's new emphasis on autonomy within the federation shows the recognition of this difficulty.

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Book Reviews

American Communism in Crisis, *1943-1957 by Joseph R. Starobin. Cambridge, Mass., Harvard University Press, 1972.—xvii, 313 pp. $12.95.*

At last, 26 years after my expulsion from the American Communist party, there appears a book that makes crystal clear that the party's inability to understand the basic concept of my Teheran thesis led it to near extinction. Under William Z. Foster's left-sectarian leadership, with his un-Marxist, anti-American phobia, the party for twelve years steadily drifted to its near demise. Today, the party is in reality nothing but an impotent sect.

No one is more competent to write the story of what happened to the American Communist movement after 1943 than Joseph Starobin, who was not only foreign editor of the *Daily Worker* but also a vocal participant in the movement of the fifties that led to the 1957 exodus from the party of most of the leading cadres and thousands of rank-and-file members and sympathizers. The party has never recovered from that catastrophe. Starobin's book, after masterfully analyzing the Duclos letter that caused my expulsion, deals step by step in great detail with the period from 1943 through 1957.

Starobin begins his book with a report made on April 28, 1956 by Eugene Dennis, the then general-secretary of the party. The report was made before an "enlarged meeting" of the party's most authoritative body, the national committee. Entitled "The Communists Take a New Look," this report was an accounting of the party's activities for the preceding ten years, namely the years *after* my expulsion. Although Dennis admitted that the collective leadership of the party during those ten years

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had made many tragic miscalculations, the man he held chiefly responsible for the disaster that followed was none other than William Z. Foster. Apparently, I was present at this enlarged meeting, for, as Starobin puts it: "Browder's name was not mentioned at the gathering, although he was surely the chief ghost present." It is important to remember that the "backward look" by Dennis came just two months after Khrushchev's sensational anti-Stalin report to the Twentieth Congress of the Soviet Communist party, a report that shook Communist parties around the world and became the catalytic agent that turned the American party upside down.

In his report, Dennis admitted that the party had badly misjudged the American political and economic scene. He criticized the "repeated overestimate of the imminence of a new cyclical economic crisis" which he said had prevailed not only in 1949 and in 1956 but also in 1945, the year that I was removed as general-secretary.

Dennis admitted that the loss of the party's influence in the trade union movement was chiefly a result of the disastrous mistake of organizing and supporting the Henry Wallace Progressive party's presidential campaign. But Dennis failed to spell out some of the details of this disaster. He failed to state that many left-wing labor leaders friendly to the party found it impossible to work with Foster and Robert Thompson, the leader of the New York State organization. Foster attempted to force left-wing labor leaders to participate in the Wallace campaign without holding any discussion with them. As a result, the party antagonized and lost the support of such well known leaders as Mike Quill and Joseph Curran. Through Foster's left-sectarian adventurism and his dictatorial methods, the party lost its influence and initiative with such powerful unions as the United Auto Workers, the National Maritime Union, the International Union of Marine and Shipbuilders, the United Steel Workers of America, and a half-dozen others. Foster's tactics even brought the CIO to the verge of a split.

Some of these labor leaders even called Foster a crackpot. But he was also a factionalist. As far back as May 14, 1929, Stalin, in a speech to the American Commission of the Presidium of the Executive Committee of the Communist International, said: "Because he [Foster] behaved first and foremost as a factionalist . . . the blindness of factionalism dulls the Party sense in people and makes them indiscriminating as to the means they employ . . . all they can think of is their own factional point of view. . . . Hence, the most important task of the American Communist Party is to put an end to factionalism and definitely cure itself of this disease." What was true in 1929 remained true for the rest of Foster's career, in 1945 as well as in the years described in Dennis's 1956 report.

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Three years after Stalin's criticism in 1929, Foster, completely misunderstanding the American scene, published a most ridiculous book, *Toward Soviet America.*

I stress these facts about Foster because neither Dennis nor Starobin bring out sufficiently the real harm that Foster did to the American party. From 1946 on Foster never ceased predicting for America major economic depressions, facism, and war against the Soviet Union. Starobin is in error when he writes that the American party in 1946 predicted, on the basis of Eugene Varga's economic writings, an early economic crisis in the United States. The prediction was based, on the contrary, on the criticism leveled against Varga's economic views by Stalin and his economic "experts." American party leaders had become aware of the Moscow dispute with Varga in 1945, the year of the Duclos letter. In fact, as a result of Stalin's criticism, Varga was ostracized and very nearly purged.

Foster's ultra-left sectarianism and his use of the Duclos letter, published in France in April 1945, condemned the American Communist party to death. Starobin's book makes very clear that the party's inability to understand the basic concept of my Teheran thesis (the subject of the criticism in the Duclos letter) led to the party's persistent belief in the theory of imminent war between the USA and the USSR, fascism in America, and a violent collapse of the American economy.

In 1945, I did not have the power to resist either the Duclos letter, clearly written in Moscow, or Foster's use of it for his "doomsday drive." Only two leaders ever succeeded in defying Stalin, Tito in 1948 and Mao Tse-tung in 1949, and both succeeded because they had their own powerful armies to support them. My only weapons were words. But it is not without significance that both Tito and Mao were denounced as "Browderites," Tito by the Soviet Communist party in 1948 and Mao by the Indian Communist party in 1949. And after 1956, even Khrushchev was denounced as a "Browderite" by the Chinese Communist party.

The term "Browderite" appeared even in some of the purge trials in Eastern Europe. In the Lazlo Rajk trial in Budapest in 1949, one of the charges was that the Rajk group "held discussion classes around ideas of Browder." In the 1952 political trial in Prague, Andre Simon begged the court to hang him because he had smuggled the ideas of Earl Browder into Communist Czechoslovakia. In both trials the victims were hanged, only to be rehabilitated in patriotic ceremonies after the Khrushchev anti-Stalin speech. My name, however, was never cleared in the Communist world.

Perhaps the most foolhardy action by the American party's leader-

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ship occurred as a result of the conviction of the leaders in the Smith Act trial in 1949. Dennis, in his 1956 "backward look" report, admits that the party so completely misjudged the character of the American scene following the Smith Act trial that it organized a self-defeating underground apparatus. The confusion and misery for the thousands that were assigned to the underground movement was so great that it became an important factor in the impact that Khrushchev's 1956 speech had on them. It was at least in part responsible for the great numbers who joined John Gates in the 1957 mass exodus from the party.

Once again my "ghost" came to haunt the party as, in Starobin's words, it floundered and "staggered to its doom." In December 1956, the New York state committee voted 25 to 8 for discarding "democratic centralism" and returning to a concept of "political action association." Was this perhaps a return to Browder's "Communist Political Association" of 1944? At the same time, a lengthy pamphlet entitled *Sources of Our Dilemma* by Chick Mason, whom I didn't know, was circulated throughout the party. This pamphlet called for a review of Browder and Browderism to see whether the party had not been in error in discarding my views. But once again, through Soviet interference plus the rapid dwindling of party membership, the remaining few rejected what was termed the "new revisionism." And Dennis, despite his 1956 report, remained as general-secretary. Thus the party became what it has been ever since, an impotent sect.

I urge all those interested in the history of American Communism to read Starobin's book, for it gives a clear picture of how a once important political movement was destroyed and how the whole complexion of American radicalism was perverted. Particular attention should be paid to Starobin's description of the mechanics of the party's underground operations, never before done in such detail. The story of how Gilbert Green, Henry Winston, Robert Thompson, and Gus Hall, the present general-secretary, went underground reads like an excerpt from a novel set in Tsarist Russia. All four went into hiding and when caught served longer jail sentences than they would have had they surrendered after their convictions under the Smith Act.

Finally, special mention must be made of Starobin's 75 pages of notes which, in themselves, make for fascinating and instructive political history.

EARL BROWDER
Princeton, New Jersey

Mr. Gus Hall, general-secretary of the Communist party of the United States, did not respond to several invitations also to write a review of this book.

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The Big Foundations *by Waldemar A. Nielsen. New York, Columbia University Press, A Twentieth Century Fund Study, 1972.—xii, 475 pp. $10.95.*

The Big Foundations deals with only 33 of some 25,000 foundations, but these, each with assets of more than $100 million, are admittedly among the most influential and important ones. The author gives a historical account of each of them, describes its development and its present practices, policies, and people, and judges its aims and performance. In a series of supplementary chapters he seeks to draw the general features of foundations, to evaluate them as a group, and to analyze the increasingly troubled relationship between private philanthropy and government. The book is clearly an ambitious undertaking. It can also be said to be timely since it endeavors to respond to questions (as it will certainly fuel suspicions) which were raised in the public mind concerning foundations by the hearings conducted on their practices by the House Ways and Means Committee in 1969. With Arnold Zurcher's *Management of America Foundations: Administration, Policies and Social Role* (1972) and Merrimon Cuninggim's forthcoming *Private Money and Public Service: The Role of Foundations in American Society*, Mr. Nielsen's book is one of three current attempts which would appear to have been prompted by those hearings to explain foundations to a doubting public. But it would perhaps be fairer to Mr. Nielsen's intent to say that in so far as his book may be described as a diatribe—and it is in part—it is addressed to the foundations themselves. His motivation would appear to have been a wish to arouse all foundations to rigorous self-examination and reform along certain specific lines that he considers desirable.

The book advances a clear thesis. It is that judged on the whole the performance of these foundations has not been good enough. It alleges that too often they have been and are controlled by timid, conservative, unimaginative people whose actions are directed more to the avoidance of controversy than to the promotion of human welfare. And it argues that if foundations do not do better (which would seem to Mr. Nielsen to mean to be led rather by professional staff than by trustees and to be more daringly and directly concerned to promote social change, particularly in the areas of race and community organization) they will have outlived their usefulness and will deserve to be destroyed by an aroused and hostile public. The questions to be asked are: Are Mr. Nielsen's accounts of the foundations accurate? Are his judgments concerning them and their people fair? Is his vision of their proper role truly insightful or does it reflect a limited, biased point of view?

One cannot begin to answer these questions in a few paragraphs. Each

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interested reader will, of course, do it for himself. I confine myself to a comment or two. I believe his case histories (whose tone is in general more sensational than scholarly) will prove to be the most vulnerable part of his book. His normal procedure is to begin with an account of the personal foibles and intellectual limitations of the individual who accumulated the fortune which a particular foundation now administers. He seems to take special delight in retailing damaging incidents which occurred a half century or more ago. Mr. Nielsen writes well and these anecdotes may give the book its greatest reader interest, but one is tempted to ask if it is fair to measure people and policies of one period by the attitudes and interests of another. And in many instances one might wonder what these early happenings have to do with the policies and practices of foundations today. Several individuals have spoken to me of questionable connections, and also of inaccuracies and partial evaluations in the case histories of foundations which they know well. These may or may not be important. In any event only detailed examinations could determine the truth of the charges. But even a quick glance at the bibliographical references to some of the chapters cannot fail to raise doubts about the author's objectivity and the depth of his scholarship.

But the basic and obviously important issue raised by Mr. Nielsen's book is of a different sort. Are his assessment of the shortcomings of foundations and his view as to what they should be doing to be accepted as normative? Have private welfare foundations, products of a capitalistic society, now become anachronistic as the federal government has moved into one after another of the areas of their earlier concern? Do those who direct foundations, through fear or excessively conservative tastes, fail to meet the human needs and social challenges of our time? Critics of foundations from the far Right charge that they are seeking to destroy the structures of our society. Others from the far Left aver that foundations have neither the will nor the interest to reform society. Mr. Nielsen's contention is rather that they fail to see or are too timid to seize the opportunities for social betterment which lie before them and so, controlled almost entirely by individuals identified with "the establishment," they in fact work to preserve the status quo and thwart change.

What he appears to be saying is that in so far as foundations focus their attention on such things as ghettoes and the distressed areas of our cities and devote their energies to eradication of the financial and political inequalities which manifestly mar society (which of course many foundations do), they deserve commendation and continued life. But in so far as they may devote attention to other levels of experience in a wider range of human need, and see merit in established institutions, and think it important to strengthen and work through them rather than to con-

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duct the work of reform themselves, they are backward-looking, fail in responsibility, and invite public scorn and hostility.

Tastes differ. Those enthralled by current liberal orthodoxies may well consider this book well aimed and insightful. For those who wonder if we have yet found the knowledge to prescribe social cures with confidence, who are more aware than Mr. Nielsen appears to be of the partiality of most human judgments, and who have doubts that satisfying answers for men are to be found exclusively or even primarily in political action, his jeremiad will seem emotional, opinionated, and even naive. These will consider much too simple and restricting his statement that if foundations are to survive they will have to change from what he maintains they have been, mere "transfer agents" simply moving funds from their accounts to those of established institutions, to become "agents of social change." And they will then conclude that the detached olympian judgment he passes on them is an ill-tempered and misdirected shot.

The Big Foundations already seems strangely dated, as if it were written several years ago in the excitement of the turbulent activism of that period. The full, balanced, truly forward-looking, fair account needed to respond to legitimate doubts that have been raised concerning foundations and the future potential of private philanthropy is still to seek.

NATHAN M. PUSEY
New York City

The Modern Supreme Court *by Robert G. McCloskey. Cambridge, Mass., Harvard University Press, 1972.—x, 366 pp. $12.95.*

Lawyers have always been prone to believe that the higher reaches of the art of Supreme Court-watching are accessible only to themselves. Persons outside the profession, who have not been schooled in the arcane lore of code pleading or the hearsay rule, have been thought to be less than completely equipped to attain the summit of this specialized form of scholarship. Political scientists venturing into this field have, in particular, customarily been met with a certain reserve on the part of those who have passed a bar examination.

If this parochial professionalism ever had any rational basis, it has surely been exploded by the coinage dealt in by the modern Supreme Court, which is the focus of this book. A knowledge of the law of bailments is not very helpful in deciding whether the religious clauses of the Constitution forbid prayers in the public schools, or whether those schools themselves are commanded by the concept of equal protection to achieve

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racial balance. A Supreme Court Justice wrestling with legislative reapportionment gets little light from his hard-won mastery of the rule in Shelley's Case. Indeed, there must be many occasions on which he wonders what the relevance of his law school education, or his years in private practice, is to the difficult issues he is currently called upon to resolve. Perhaps, he might even speculate, it was not through inadvertence that the Framers omitted any explicit requirement that members of the Court be members of the bar.

It was the peculiar virtue of the author of this book—Harvard's late and greatly lamented Jonathan Trumbull Professor of American History and Government—that he brought to bear upon the Supreme Court the political scientist's concern with its institutional role in the governance of the country, its place in the *total* framework of government. His training as a political scientist and historian arguably equipped him far better to come at the Court from this quarter than would be true of most lawyers, encumbered as they tend to be with purely professional preoccupations. At all events, Professor McCloskey's long and close observation of the Supreme Court in action over the last thirty years has borne fruit in this thoughtful and valuable book. Because of his untimely death, it must serve as his last statement on the subject, to the future impoverishment of all who share his sense of the profound importance of the Court.

The book in reality deals with three courts in that composite we think of as the modern Supreme Court, which came into being just prior to World War II—the Stone, the Vinson, and the Warren Courts. The material on the first two appears here for the first time; that on the third had been published earlier, mainly in the law reviews. It is the Warren Court which, not surprisingly, preempts the major span of Professor McCloskey's attention, although his discussion of the earlier two perceptively identifies the degree to which their work anticipated and furthered the striking developments which lay ahead. That achievement was not inconsiderable, particularly when it is remembered that the Stone Court's life coincided with the war, and the Vinson Court functioned coincidentally with the distractions of its immediate aftermath. Both were subject to problems and pressures unique to the trauma of world conflict, but which had visibly subsided or changed their character by 1953, when Governor Warren became Chief Justice.

One of the most useful things in this book is an effort to formulate standards by which the Supreme Court's performance in reading the Constitution may be evaluated. The author comes up with three: competence in working with, and within, the historical and technical traditions of the law; choice of ethical values; and caution in estimating power capabilities. Others might make a somewhat different list, but

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these are helpful aids to analysis of why it is that one reacts positively or negatively, either to a particular decision or to a sustained course of constitutional interpretation. Applying his own tests, Professor McCloskey gives the Warren Court a comfortably passing grade on the first score, and high marks indeed on the second. It is the third which finds him troubled and uncertain—a state of mind which he shares with many who, like himself, regard the Court as an essential, albeit a fragile, part of our polity.

CARL MCGOWAN
United States Court of Appeals

Roosevelt and Pearl Harbor: A Great President In A Time Of Crisis *by Leonard Baker. New York, The Macmillan Company, 1970.—viii, 356 pp. $8.95.*

Leonard Baker asserts in the preface to this interestingly written study that, although "a large number of well-researched books have been written about certain aspects of the events leading to Pearl Harbor . . . the story of Franklin Roosevelt and the coming of Pearl Harbor remained untold" (p. vii). That statement will at the very least irritate several hundred writers, each of whom believes he has provided *the* account of FDR and the coming of the war. Further, if "story" is taken to mean explanation, Baker has done no better than the legion that preceded him in offering a definitive account of Roosevelt and United States entry into the Second World War. Indeed, in some ways this book is a regression to a familiar and largely outmoded interpretative schema. To Baker, Franklin Roosevelt was the "great educator," a man of decisive personality, one who early recognized the significance of the Axis threat and who worked unceasingly to prepare the American public for a war he believed both necessary and inevitable. Total acceptance of the "farsighted statesman-determined leader" school of interpretation regarding FDR's actions in foreign affairs is no longer tenable. Recent scholarship, much of which was available by the time Baker opted for this archaic interpretation, has advanced beyond explanations of Roosevelt-as-saint or Roosevelt-as-sinner. FDR is now treated as a human being, beset by many and complex problems—some of which he handled brilliantly, some he fumbled, and a great many he merely postponed.

Baker does much better with the "time of crisis" that forms the second major theme of his exposition. Indeed, viewed as an effort to narrate for a general audience the responses of the administration and the American

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people to the crises of the twelve months preceding December 7, 1941, *Roosevelt and Pearl Harbor* deserves considerable attention. The treatment of the political and psychological context within which the administration's policy took shape is always competent and at times truly brilliant. There nowhere exists a comparable distillation of the military preparedness-munitions production issue; and Baker's presentation of domestic politics, while brief, is cogent and balanced. Baker has done thorough research in the mountainous secondary literature, and his study also reflects selective but careful use of an impressive number of manuscript collections. The result is solid history, a book that, while not the definitive "story" (as if one were possible), may well survive as the best narrative treatment of Roosevelt's journey to Pearl Harbor among a superb field of contenders for that distinction.

THEODORE A. WILSON
University of Kansas

Launching Social Security: A Capture-and-Record Account, 1935-1937 *by Charles McKinley and Robert W. Frase. Madison, University of Wisconsin Press, 1970.—xxiv, 519 pp. $12.95.*

In November 1935, Professor Charles McKinley, at the request of the Social Science Research Council, embarked upon a capture-and-record study of the administration of the newly enacted Social Security Act. After six months he left for another appointment, returning only for three months toward the end of 1936. His place was taken by a younger man, Robert Frase, who worked on the project until March 1937. The two reports were combined in 1941, but in deference to the objections of some of the principal characters in the story, publication was postponed until 1970, by which time McKinley had died. At this distance in time, checking on the accuracy of the reported events is not easy but in at least one instance in which this reviewer was involved her memory and records differ greatly from the events as recorded in the book.

The SSRC had hoped that the study would "throw light on the special problems encountered in a new field of activity in the United States but would also help to determine whether there are discernible principles of public administration." The book more successfully achieves the former, than the latter objective.

Students of the social policies of the New Deal will find a fascinating account of the organizational and staffing problems, the political pressures and policy issues faced by the Social Security Board in launching the new federal-state unemployment insurance and public assistance pro-

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grams and in implementing the tremendously important old age insurance system (though one could have wished that the problems connected with the assignment of social security account numbers had not been treated at such exhausting length).

But administrative principles are another story. The senior author admitted that his criteria for selection of administrative experience was dictated in large measure by his lack of specialized knowledge of federal administrative procedures and practices and of European administration of comparable programs, and in the end he cast his net very wide, covering almost every aspect of the board's work in the first fifteen months, except for routine activities and procedural details. Concentration on a more limited number of aspects or concepts of administration and an organization of the material on the basis of these rather than of programs might have contributed more to social science.

The authors are commendably modest about their conclusions. They believe, however, that the board form of organization was a mistake, leading to delays, undesirable publicity about differences of opinion, and a lack of clarity as to what was "policy" (appropriate in the authors' view for board attention) and what was "administration" (with which, they hold, the board should not be involved). Yet in the inauguration of a series of major programs, so novel, so vast in terms of numbers of people affected, and so charged with potential political significance, especially in the field of federal-state relations, a good case can be made for deliberate and careful consideration of the many-sided issues raised by almost every action in those early months, whether "policy" or "administration" and for this purpose a board consisting of three men with such diverse talents and experience was probably an advantage.

The authors also criticize the board for its continuing involvement with personnel appointments and for making excessive use of the "expert" category instead of relying on civil service registers or recruiting from among persons with civil service status in old-line federal agencies. It is true that this policy exposed the board to congressional importuning for patronage appointments, not all of which could be resisted although the number of purely political appointments was surprisingly small. Nor, given the novelty of these federal programs, was it as if there were a pool of experienced federal employees on which to draw or appropriate and established examination criteria.

The authors also criticize the organizational structure established by the board and especially the elevation of a number of functions such as accounts and audits or information services to the status of bureaus, thus weakening the authority of the executive director. Yet it seems clear that the major motive was to establish a salary classification for the in-

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dividuals who were to be in charge of these activiites that would make it possible to recruit highly competent persons. And with the advantage of hindsight, who is to say that the board's great concern for staffing witth competent persons who wholeheartedly believed in the new programs was a mistake? The agencies, and notably the Social Security Administration, which have administered the various programs inaugurated by the Social Security Act of 1935 have been outstanding for incorruptibility, high professional competence, devotion to the spirit of the legislation, and concern for the people in whose interests it was passed. What the board did in those early months was to establish a valuable tradition.

EVELINE M. BURNS
Columbia University

The Rise of Warren Gamaliel Harding, 1865-1920 *by Randolph C. Downes. Columbus, Ohio State University Press, 1970.—x, 734 pp. $17.50.*

Professor Downes has written the longest book to date on one of the least significant of American presidents. It is solid political history. Neither Nan Britton nor Carrie Phillips are mentioned. Mrs. Harding herself is barely visible. There are no speculations here on childhood experience or growth processes. No probings of the inner man are attempted except in terms of political expediency and Harding's admittedly unknowledgeable opinions. The entire first half of the book concerns the endless details of Harding's work on the Marion *Star* and his rise to eminence as speechmaker politician in Ohio, "The Mother of Presidents."

But at midpoint, the patient reader will be most pleasantly rewarded by a thesis, hinted at by other Harding biographers, but dealt with by Professor Downes with relentless documentation, namely, that Harding never was a "dark horse" candidate. From late 1919 to the Republican National Convention in June 1920, he conducted in secrecy a campaign for delegates' second and third choices which, if deadlock came, would assure him the nomination. This strategy encouraged "a multiplicity of first choice candidates so that he could concentrate on second choice promises from all of them" (p. 345). Harding's manager, Harry M. Daugherty, played a leading role in the planning and implementation of this novel approach to president making, right down to the folksy image, the front-porch McKinley setting, and the heavy doses of patriotic rhetoric. So that when Frank O. Lowden and General Leonard Wood reached stalemate on the sixth ballot, Harding simply sat it out with more sec-

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ond-, third-, and fourth-choice pledges than any other candidate. In the end, there was no "smoke-filled room"—there was only arithmetic.

The balance of Professor Downes's work covers the Republican campaign of 1920, and again the author renders a service to history by underscoring the astute planning and precise use of the media by talented advertising experts such as Albert D. Lasker and William Wrigley, the chewing gum king. Downes concludes that Harding was "a much-managed candidate but the public was a much-managed public, and that they enjoyed it" (p. 495).

In regard to the League of Nations as the greatest issue of the campaign, Professor Downes paints Harding as tactician par excellence. Democrat James B. Cox stood with Wilson and the League. Harding, by proposing instead an "association of nations" tied more or less to a world court and disarmament, provided a "forum" which gave him flexibility, drew to him support from eminent pro-League Republicans and huge numbers of Progressives who possibly would have voted with the Democrats. This was the cornerstone of the campaign strategy—to hold out some hope always that the intensely conservative Harding would magically return to the policies of the martyred Roosevelt, but at the same time continually to remind Progressives that so long as they were apart from the GOP, patronage would be denied them.

Professor Downes's summation of Harding's ideological principles bring the 1920s and the 1970s into chilling perspective. Harding's views "consisted essentially of tariff-Americanism and a combination imperialist-protective attitude toward other nations. Both were based on a lack of knowledge . . ." (p. 638).

DEAN ALBERTSON
University of Massachusetts, Amherst

US Expansionism: The Imperialist Urge in the 1890s *by David Healy. Madison, University of Wisconsin Press, 1970.—x, 315 pp. $10.95.*

A few years ago, David Healy came out with his first book, *The United States in Cuba, 1898-1902: Generals, Politicians, and the Search for Policy* (Madison, 1963), which stressed the supremacy of political over economic factors in the early decisions concerning the American occupation of Cuba. Now he has written a broader analysis and interpretation of "expansionism" in the nineties, the message of which is less clear cut. His intention (in no place stated explicitly) appears to be to push aside

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all monocausal interpretations to make room for a frankly eclectic explanation of the ideas and forces that impelled the United States on an imperialist career.

Healy seems to be saying that the United States became imperialist for the following reasons: it was the thing to do in the 1890s; America's leaders thought expansion was in the nation's strategic interest; they believed war and imperial duties would both improve the tone of American society and regenerate the lives of the subject peoples themselves; they regarded it as the duty of the U.S. to help supply law and order to the disorderly districts of the world; and they thought that the exigencies of the American economy required the expansion of Uncle Sam's power and presence in the less developed regions of the world.

Two of the earliest consequences of this drive were the abandonment of a separate-hemispheres foreign policy outlook in favor of one that looked upon the world as a single, interrelated whole and the growth of the idea that it was desirable for Americans to export their outstanding qualities and historic mission to others less fortunate.

There is nothing startling about any of this, and Healy's research has turned up no surprising new information. The structure of this book, involving an overly contrived interspersing of general analyses and biographical illustrations, is more awkward than enlightening. What some of Healy's chapters are supposed to prove—especially those on James Harrison Wilson and Elihu Root—is by no means apparent. The book is most disappointing, however, in its author's reluctance to be more explicit about his conclusions and to evaluate the *relative* importance of the sundry factors that produced American expansion. An eclectic viewpoint, however appropriate, shouldn't rule out the obligation to discriminate when possible between major and minor causal factors.

Despite these limitations, *US Expansionism* is a valuable and stimulating book. Healy uses felicitous prose to review the attitudes not only of old standbys like Root and Theodore Roosevelt, but less familiar figures such as Wilson, Charles Denby, and Charles Conant. His comments on the connections between economic and political ideas are valuable, and even more so is his demonstration that members of the expansionist elite who were predominantly power-minded and preoccupied with international political rivalries manipulated economic issues, such as trade expansion, for their own purposes. It has long been suspected that some of the duty-and-glory talk disguised other motivations; why couldn't appeals to trading profits have been used in the same way? Finally, Healy has interesting things to say on dozens of assorted topics—the changing conception of the American Mission, the distinction between

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imperialism and expansion, the blending of nationalism with religious revivalism, the area of common ground held by imperialists and anti-imperialists, and many others.

The book is well printed, with footnotes at the back along with a lengthy bibliography.

ROBERT L. BEISNER
The American University

Dollars for Research: Science and Its Patrons in Nineteenth-Century America *by Howard S. Miller. Seattle, University of Washington Press, 1970.—xi, 258 pp. $9.50.*

Professor Miller has given us a pleasant introduction to the history of many of the private observatories, botanic gardens, museums, scientific schools, and research funds of nineteenth-century America. Since Miller writes well and has an eye for colorful detail, his book makes enjoyable reading, but his selection of topics is less felicitous. They are chiefly the old stories of the establishment of the Smithsonian Institution, of Louis Agassiz's fund-raising prowess (and by contrast Asa Gray's more modest achievements), of B. A. Gould's misadventures at the Dudley Observatory in Albany, of J. D. Rockefeller's money and W. R. Harper's administrative talents at the University of Chicago, of E. D. Cope's and O. C. Marsh's paleontological battles, of the Allison Commission's debates on government science, and, finally, of the founding of the Carnegie Institution of Washington.

Miller has chosen a large and scattered subject. Although he has used 29 manuscript collections (described at the end in an extensive essay on sources) to document his vignettes, much of the work has already been published elsewhere, and his episodic treatment leaves the impression of only skimming the surface of this important subject. He highlights the drama of the philanthropists' eccentricities and of opening-day ceremonies, and he underplays the frustrations and limitations of philanthropic support. Many projects which had glorious beginnings later foundered for various reasons which Miller does not pursue systematically. This side of the history of philanthropy deserves a more sustained analysis than Miller gives us.

It is hard to determine what Miller's stance on his subject really is. At one point, he expresses surprise at how much support there was for science in America (p. 182), but more often he deplores how little there was. Comparative quantitative data is surely hard to come by, but some

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indication of the amounts going into science rather than into other charities would have been useful. Without it, we are hardly able to generalize about the adequacy or inadequacy of philanthropic support for science.

But Miller does offer some observations on the nature of philanthropic support of science in America in the nineteenth century. It was uncertain and sporadic; it concentrated heavily on the sciences of astronomy and botany, emphasizing buildings and instruments rather than research funds for the support of scientists (although this last imbalance may be at least partly due to Miller's omission of endowed professorships as sources of support, even though most scientists in America at this time were professors). These observations lead Miller on occasion to comment on the motivation of philanthropists, but he shies away from further analysis since their motives were so diverse and so idiosyncratic.

In this, as in other areas, one might wish that Miller had been more rigorous and critical. For instance, Miller tells us that as the sciences grew increasingly complex and esoteric, they became more and more remote from the comprehension of the philanthropist. Plausible as this seems, Miller's own examples show that there was no diminution of philanthropic support over time, and, in fact, quite the opposite took place. O. M. Mitchell was able to raise about $10,000 for popular astronomy at the Cincinnati Observatory in the 1840s, but George Ellery Hale around 1900 was able to attract about $1,000,000 from the Carnegie Institution for hard-nosed research in astrophysics. It would seem that the philanthropists were more than able to keep up with the specialization of the sciences.

Although Miller's contribution to scholarship might have been greater had he been more critical and rigorous in his reasoning and data, still his book is an appealing one that may serve to guide others who may wish to pursue the subject further.

MARGARET W. ROSSITER
Brown University

Politics, Position & Power: The Dynamics of Federal Organization *by Harold Seidman. New York, Oxford University Press, 1970.— x, 311 pp. $7.50.*

At a time when popular attention seems to be focused overwhelmingly on the results of the legislative process, Harold Seidman effectively re-

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directs our concerns to administration as an equal and vital part of the governmental process. Economy and efficiency are not the prime concerns; "the basic issues of Federal organization and administration relate to power: who shall control it and to what ends?"

Mr. Seidman wrote this book during his sojourn as scholar-in-residence with the National Academy of Public Administration. With insight that permits him to cast the experiences of his own career—and the lessons of an intimacy with the history of an era—in new terms, Mr. Seidman explains the intricacies of the politics which is administration. With exposition rather than argument, he lays out his conceptual movement beyond the old orthodoxies that separated politics and administration and the dogmas of the Hoover Commission, and continues to recount the ecology of administration—Congress, the White House, the executive establishment, intergovernmental relations.

One cannot, at the same time, describe American representative government and federalism adequately enough for pragmatic action and simply enough for comfort. Running throughout the book is an acute awareness of the pluralistic nature of our government: "It is highly misleading to speak of *the Congress,* as if it were a collective unity"; "President Nixon cannot ignore the trend toward fractionalization and compartmentalization of Federal programs that has made the subgovernments powerful and arrogant. . ."; "To speak of the Federal bureaucracy as if it were a homogeneous entity is obviously most misleading"; "The Federal system is not a single system, but a loose grouping of relatively autonomous confederations of Federal, State, and local professional agencies."

Rampant pluralism is the result of many attempts to solve particular problems for particular claimants. It is, also, the source of many of the problems that tend to undermine confidence in government's ability to help in solving society's problems. Governors find themselves unable to commit their states' resources often because of the structure of their government atomized by special interest constituencies reinforced by the special grants and incestuous attitudes of the professionals from the federal level. The poor and politically powerless, enmeshed in their communities in independent quasi-governmental citizen action funded by federal funds, discover that their influence has faded into a convenient excuse for a governor or mayor to evade the tough political problem of reallocating resources.

Mr. Seidman's chapters focusing on the states, "Cooperative Feudalism"; the special devices created for coordination, "Coordination: The Search for the Philosopher's Stone"; and the agents that confuse public and private, "Advisory and Intergovernmental Bodies: Twilight Zone" deserve a special place on the reading list of every student of American government. As succinct a treatment of the same melange from a point of

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view developed with local governments would be another major contribution which the National Academy of Public Administration might well encourage.

For many authors, the past can be "explained" and made to fit descriptive hypotheses. As they move to the prescriptive, the judgmental leaps may fail to carry the reader, or they may become banal. In a few instances in this book, the reader gets the feeling that he missed a transition to a conclusion. But neither the style nor the text is banal.

In his "Concluding Observations," Harold Seidman pulls together the philosophy upon which his book is premised: The general welfare has been ill served by fractionalizing forces; strategy should aim at an administrative and political system in which power is so balanced that each class and section must make concessions to the other. Then he concludes, not with the rules for judging program and administrative design, but with the questions that should be asked. Wisdom has often come in questions. Certainly, program design might be much more sound if these questions served as a check list.

JAMES A. NORTON
Cleveland, Ohio

The Politics of Disorder *by Theodore J. Lowi. New York, Basic Books, Publishers, 1971. —xxii, 193 pp. $6.95.*

This new work by Lowi will yield few surprises to readers of the author's earlier book, *The End of Liberalism.* These loosely connected essays restate and extend Lowi's previous contentions without managing, I fear, to clear up the contradictions and inconsistencies that marred his earlier work.

"Interest-group liberalism," that is, a politics of compromise and bargaining between the major politically relevant social and economic interest groups, continues to be Lowi's *bête noire.* Such bargaining politics leads, so he contends, to a devolution of central authority. What is needed is a restoration of the rule of law. The mutual adjustments upon which the regime of interest-group liberalism rests stultify rational political action and dissolve the polity into congeries of self-interested groups loosely coordinated by institutionalized power brokers. Whatever its merits in the past, he asserts, this political system now has come to the end of its tether. If the otherwise irremediable drift into continued politics of disorder is to be halted, we need to return to a restored Constitution, a juridical democracy closer to the ideals of civic textbooks. We need a structure of formal government and law majestically unconcerned with the vagaries of contentions among interest-oriented citizens and

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their group-centered pursuits.

So far it would seem that Lowi's thesis has a great deal of affinity with Hayek's praise of formal law as well as with a Hegelian reliance on the moral superiority of state action. Like Hegel, Lowi seems to dream of a polity in which the "shared particular interests," the "systems of need," the subjective tastes and private preferences of ordinary citizens are transcended through the disinterested actions of a governing and rulemaking elite austerely commanding the levers of the state in accord with the Rule of Law.

Yet matters are not so simple. While much of his thought would indeed place Lowi in the conservative camp, the nevertheless wishes to be considered a thinker of the Left rather than of the Right. Hence he attempts to supplement his previous attacks on what he considers the deadening effects of bureaucratized and rigidified interest-group politics by a confident reliance on the rejuvenating qualities of social movements as yet untainted by the deadening effects of routine and habitual adjustments. But that is most unconvincing. Young movements, just like young men, do not stay young forever. As they grow into maturity they hardly escape a measure of routinization. To counterpose social movements to social groups, or the bliss of young married love to the "routinized love" of maturity, is just like arguing that "you can't trust anybody over thirty." It is a pathetic fallacy which can hardly serve to underpin a new theory of politics. What one is left with is a kind of conservative utopia dressed up with tattered remnants of the cult of youth and spontaneity.

Lowi has an exceedingly sharp intellect and he commands a fine expository style so that it is always a pleasure to read him, even if one is compelled to dissent from the main drift of his argument. Moreover, certain chapters of this book, more particularly the ones in which he passionately defends the university against those who wish to pervert its mission in the name of "relevance" or technocratic requirements, are a joy to read.

I, for one, await with a measure of anticipation the future work of an author who, despite all his shortcomings, has managed to break out of the habitual groves of liberal political inquiry. But I also hope that his future work will not be marred by *obiter dicta* such as, "No alleged enemy of democracy has the capacity to weaken democracy." Before he sits down to write his next book it might be worth Lowi's while to study the politics of Weimar Germany as well as the implications of the rejection of pluralism in the pre-Weimar Rechtsstaat.

LEWIS A. COSER
State University of New York at Stony Brook

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Civil Disobedience: Conscience, Tactics, and the Law *by Carl Cohen. New York, Columbia University Press, 1971.—222 pp. $7.50.*

Our day has been characterized by social disturbances of all kinds. The civil rights, antiwar, and student movements of the sixties and now the agitations associated with such movements as women's liberation have raised questions about the desirability and legitimacy of political tactics in general and of civil disobedience in particular.

In this clearly written volume, Professor Cohen has provided us with a rather concise statement of what civil disobedience is and is not and of its types, relations to other forms of action, and justifications. The writer also examines typical arguments against it, deals with its relation to free speech, and passes on the relevance of the Nuremberg judgments for its vindication. Throughout the volume, he provides us with valuable case histories of civil disobedience and invites us to examine their merits and demerits. Both those familiar with civil disobedience and those unacquainted with it can profit from this book.

Cohen distinguishes, as do many others, between "direct" and "indirect" disobedience—the former being deliberate violation of a law deemed to be unjust, the latter violation of some statute which may be regarded as legitimate in itself but the violation of which is thought necessary as a tactic to oppose some social evil which might otherwise be difficult to attack. The author defends both types of disobedience under certain circumstances. Against Howard Zinn's argument that if the statute is unjust, the civilly disobedient need not accept the penalty, he argues that "miscarriages of justice do not in themselves, justify the abandonment of a legal system, or the abandonment of normal procedures whenever one accused under them complains of injustice" (p. 90).

Among the most vital parts of the volume are the chapters in which the writer seeks to vindicate civil disobedience in both positive and negative terms. He points out the difficulties of "higher law" justifications and appears to rest his case primarily on "utilitarian" vindications. Although he is certain that civil disobedience cannot always be justified, he does think that the arguments which attempt to exclude it altogether are invalid. He rejects such arguments as: civil disobedience implies contempt for law; it supposes the primacy of selfish interests; it means that its practitioners take the law into their own hands; it undermines respect for the law; it is necessarily self-defeating and cannot be justified when "lawful" channels remain open; and it "subverts" the democratic process.

In his chapter on civil disobedience and free speech, he repudiates the

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notion that disobedience is a kind of "symbolic" speech and therefore protected by constitutional guarantees. When he comes to discuss the Nuremberg judgments, he rightly points out the ambiguities and difficulties involved and suggests that the utilization of Nuremberg to justify civil disobedience is not as self-evidently right as many of its exponents assume.

The spirit of the whole volume is perhaps best epitomized by the last paragraph:

> If law is the skeleton of justice, however, it is not its heart. After the context is known, the laws distinguished, and the levels of authority and conflicts of jurisdiction made clear, the moral task remains—that of determining whether, in that context, given those facts, a given law ought deliberately to be broken. That is what civil disobedience is all about (p. 214).

MULFORD Q. SIBLEY
University of Minnesota

The Three Worlds of Economics *by Lloyd G. Reynolds. New Haven, Yale University Press, 1971.—xiii, 344 pp. $12.50.*

In discussing the question of the convergence of modern capitalist and socialist systems, Lloyd G. Reynolds writes: "Economists would tend to emphasize methods of resource management, that is, the institutional organization of the economy." In this respect there may be convergence, "partly because some economic problems are common to socialist and capitalist countries," and "some methods of handling these problems work better than others" (p. 297).

This view of economics provides a narrow base for Professor Reynolds's survey of capitalist, socialist, and "less developed" economies. It is evidently against the rules to mention politics or the aims of policy, conflicts of interest, or the relations of classes within society.

It seems to be against the rules even to mention property. The author somewhat hesitatingly admits that (allowing for free social services) the distribution of income in the USSR is less unequal than the distribution of wage and salary incomes in the West (p. 93), but nothing is said about income from rent, interest, or stock appreciation.

In connection with LDCs (less developed countries) it is admitted that income distribution is probably more unequal than in the West, but the point is dismissed for lack of accurate statistical measures, and nothing whatever is said about property in land and business, nor about what influence the propertied classes have upon national policy, nor about how their household consumption affects the composition of output and

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the composition of imports in countries which are supposed to be struggling to develop. The author does, indeed, mention that there is "unused saving capacity," but he attributes this not to waste and luxury but to the lack of "secure assets at yields reflecting the scarcity of capital in the economy," in which savings could be placed (p. 105). The limit to saving through taxation is attributed to the danger of inflation (p. 106), not to the political influence of those who might be taxed.

It seems that it is against the rules to mention history, also. For instance, the centralized control of the economy in the USSR, investment planning, and the emphasis on heavy industry are treated as some kind of idiosyncracy of socialism, not as the product of a revolution which destroyed all former organs of economic initiative and left the country weak in a menacing international situation.

Similarly, it is noticed that it is unfortunate for many LDCs to be dependent on the fluctuating market for a single export product (p. 109), but nothing is said about how this situation arose from development in the colonial period.

The discussion of capitalism is even more apolitical, and, indeed, it is not always clear whether the discussion is about a modern capitalist nation or about an old-fashioned textbook one. (Consider for instance the observation on page 130: "Only in the vast American market is atomistic competition feasible for any considerable number of industries.") The clue to the whole argument is to be found in the section on "The Usefulness of Western Economics." Here it becomes clear that while Professor Reynolds feels obliged to recite the slogans of the textbooks, he does not believe in them. He does not believe that there is atomistic competition in America; he does not believe that there can be economics without value judgments; he does not believe in consumer's sovereignty or in the pure theory of international trade. He pleads for economics to be released from the shackles of Robbins's definition about "scarce means with alternative uses." (He does not seem to realize that Keynes released us 35 years ago.) He would like to explore the sources of technical change, the problem of population, the emancipation of women, the relative costs of public works when organized under the profit motive and by means of community self-help under community control. It is true he does not venture very far into politics and not at all into history, but he longs to free himself from the irrelevant assumptions of formal, mathematicized economic theory so that he could discuss the problems of the three worlds as they really are. Then why does he not do so?

JOAN ROBINSON
Cambridge, England

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The Cruel Choice: A New Concept in the Theory of Development *by Denis Goulet. New York, Atheneum, 1971.—xv, 362 pp. $12.50.*

Economic development can no longer be considered an unqualified social blessing. The industrial nations have discovered that the environmental and moral costs of sustained material "progress" are heavier than had been thought, perhaps heavier than should be borne, or at least after a certain minimal level of comfort has been achieved. This discovery raises immediate questions about the less developed world: should these poor countries follow the same path as their predecessors? Must they make the same mistakes? How can development be given a truly moral and humane basis?

Denis Goulet attacks this broad range of questions in his interesting and imperfect book, *The Cruel Choice.* His main focus is on the less developed countries and on the ethical choices they face. Development, in his view, "is not a cluster of benefits 'given' to people in need, but rather a process in which a populace acquires greater mastery over its own destiny" (p. 155). Given this definition, it is clear that greater and greater accumulations of goods have little to do with the matter. Certain quantitative gains are required at the beginning, but the process is more aptly described by such terms as "maturation," "humanization," a "qualitative ascent." The goals of development cited by Goulet are *life-sustenance* (a certain level of material provision is necessary so that life can go on under bearable conditions); *esteem* (whether rich or poor, all men need a sense of their own worth); and *freedom* (an expanded range of choices, ideally for the exploration of new possibilities and for self-actualization). These goals in turn suggest certain ethical strategies for development, including broad popular participation in national planning and decisionmaking.

The problem with Goulet's analysis is not so much that it is wrong as that it is arbitrary. His three developmental goals are fine, perhaps, but surely there are other possibilities. One might argue, for example, that outstanding cultural achievement should be the hallmark of the truly "developed" society. But if so, can we be sure that the broadest possible popular participation will lead toward and not away from that end? His treatment of freedom is especially weak, partly because at one point he defines it so broadly that it includes "security" as a special case, but also because he seems ready to abandon a society's freedom without sufficient struggle. If a populace, wishing to avoid decisions, chooses to delegate its responsibilities to its "hierarchical leaders," well, "so be it"! This is potentially dangerous doctrine: under it, slavery

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and freedom could become virtually indistinguishable.

The book is also marred by its failure to relate the qualitative side of development to its crude but nevertheless insistent quantitative dimension. In one of his later chapters, Goulet rather abruptly takes up certain numerical aspects of the world population problem. He makes the case for population control so overwhelmingly that one wonders why he did not show how such control is specifically related to his over-all goals of sustenance, esteem, and freedom and, even more important, how the achievement of such control will be advanced by the ethical strategies for development he is advocating.

The Cruel Choice is, then, a flawed treatment of a highly important range of questions. If social scientists have learned anything in the two hundred years since the Industrial Revolution, it is the danger of assuming that social processes are inevitable and that the problem of "values" is simply one of adjusting our outlook to irresistible historic forces. Denis Goulet does not make this error. He attacks the ethical problem head-on, with both courage and compassion. If his solutions are never quite satisfactory, this mainly measures the scale of the task he has undertaken.

RICHARD T. GILL
Harvard University

Agrarian Policies and Problems in Communist and Non-Communist Countries *edited by W. A. Douglas Jackson. Seattle, University of Washington Press, 1971.—viii, 489 pp. $15.00.*

The volume contains papers and comments from a conference held in Seattle in August 1967. It contains 16 articles, with one or two discussants' comments for each (except the last one), by a total of 29 contributors. The preface is dated April 1969, but the printing was accomplished in 1971. Such delay is serious for a book addressed to the research and policy frontier of the day. In some articles, data have been brought up to 1967, but most tabulations stop in 1965 or earlier. The articles most directly concerned with recent developments were, therefore, out of date before they reached the general public. The volume is handsomely printed and well edited, but the question must be raised whether a quicker, more rough-and-ready mode of publication would not have been more rational.

This remark does not apply to the same degree to all the articles; the more long-term analyses retain most of their value. The article by Alec Nove on "The Decision to Collectivize" in the USSR offers an authori-

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tative and in many ways definitive condensation of a large body of research on a series of events concluded long ago. Karl August Wittfogel's enormous overview of Soviet and Chinese agriculture in their historical setting also has lasting value as a convenient summary of the author's voluminous published work. Werner Klatt's concluding article on "Successes and Failures of Communist Farming" is a concise, well done summary and outlook on long-term problems.

Most of the articles are more directly addressed to the problem of applying Communist experience to low-income, "third-world" countries and for this reason the emphasis on recent data sometimes causes early obsolescence. This is especially true of the four articles on Communist China by Leslie C. Kuo, Anthony Tang, Lawrence J. Lau, and Shigeru Ishikawa. The difficulty is here compounded by the scarcity and fragility of the data. Much of what these writers say has been superseded by the weighty analysis of Kang Chao in *Agricultural Production in Communist China 1949-1965* (Madison, 1970).

The Soviet studies are not equally vulnerable, but the difficulties of some of them raise, *a fortiori*, even greater doubts about the validity of any ready-made conclusions about China. Karl-Eugen Wädekin's treatment of "Kolkhoz, Sovkhoz, and Private Production in Soviet Agriculture" is mainly a straightforward narrative based on official sources and is for good reason wary of the ambiguities of these sources. Much less satisfactory is the analytical attempt by Eberhard Schinke on the employment of factors in Soviet agriculture. Any productivity analysis requires more strict conceptualization than we are offered here. Productivity is judged too much on the fixed factors—capital stock rather than flow (depreciation, fuel, etc.); the treatment of draft power resources is outright misleading. The article by Henri Wronski on Soviet consumer cooperation has many interesting observations but leaves the reader quite puzzled.

The attention to Communist countries is focused mainly on the USSR and China. A useful study by the late Jerzy Karcz (to whose memory the volume is dedicated) deals with certain aspects of new economic systems in Bulgaria and Czechoslovakia. A comparative study by Konrad Merkel on East and West Germany appears too impressed by the returns-to-scale argument in farming and thus falls for some of the Communists' exaggerations. A better perspective is offered in the broadly comparative article by Otto Schiller.

Other non-Communist experiences are presented from Japan, Korea, and Taiwan. The Korean study by Ki Hyuk Pak is hampered by the lack of information from North Korea. Both the South Korean study and its counterpart on Taiwan by S. C. Hsieh suffer from the difficulty of as-

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sessing the impact of foreign aid, which has been very important in both countries. The Japanese study by Fukuo Ueno is an in-depth analysis of certain development aspects in a limited local area. It is an interesting example of the kind of studies that will be needed as a basis for more definitive evaluation of the influence which alternative agrarian systems may have on economic and social development.

The book contains a wealth of information. Needless to say, it is far from being the final word on its vast subject. On balance, it is well worth having, and it is a pity that it could not have been published faster and cheaper.

FOLKE DOVRING
University of Illinois

Foundations of Modern Historical Scholarship: Language, Law and History in the French Renaissance *by Donald R. Kelley. New York, Columbia University Press, 1970.—x, 321 pp. $10.00.*

In this important book, Professor Kelley explores the great revolution in historical awareness that was accomplished by the scholars of the Renaissance. Although the roots of this change derived from Italy, the main achievements came in France, where a number of traditions intersected and combined. The new historiography, moreover, was not the work of narrative historians who, at the beginning of the sixteenth century, still wrote of "dynasties and wars." Essential to historical awareness is the idea that the world of institutions and customs is not a fixed backdrop for great actions, but is relative to time and place and subject to processes of change. As Professor Kelley shows, this perspective emerged as a kind of by-product of the great French school of legal humanism which applied the high philological technique of the older humanism to an ever-widening range of legal sources.

Professor Kelley's narration of this change is comprehensive and rich in detail. It is a theme on which he has worked for many years, and his erudition is remarkable. His natural starting point is the appeal by Lorenzo Valla for a humanist reform of legal studies, and it is in the thought of Valla that Kelley also finds the most revealing statement of the philosophic and epistemological assumptions that underlay the later movement—the idea of going to the things themselves, or more properly to human things, by studying the content and the forms of human discourse. The application of this program to something that comes close to being history was the work of the legal humanists from Bude and Alciato on

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through Cujas, who overthrew the scholastic methods of the medieval Roman lawyers for a philological study of the texts, and then attempted to push beyond the *Corpus Juris* to reconstruct the original form and meaning as well as the historical phases of the Roman legal sources. In the treatment of this school a special place is accorded to François Baudouin, who was the first to call for a full fusion of legal and historical studies in order to create a universal history. But the way to a broadened view of history was actually prepared by learned legal commentaries, like those of Dumoulin on the customary law of France, for in this enterprise historical questions like the origins of feudal law or of the privileges of the Gallican Church could hardly be avoided. The culmination, which is taken up in the lengthy last section of the book, was the attempt of the great French school of antiquarianism, led by Pithou and Pasquier, to reconstruct the history of medieval France in order to create a truly national history.

Professor Kelley thus concludes that historicism—in the sense that the world of human institutions is perceived as profoundly historical—was already developed in the sixteenth century. He is a bit reluctant in admitting that Renaissance historiography was often in the service of syncretizing universalism, as for example in Bodin, and tends, in my opinion, to underestimate this other impetus, perhaps because he clearly disapproves of it. However this may be, he has definitively shown that the historicist element was present and was powerful, and this is an important finding.

One weakness of the book perhaps is that certain recurring themes—like the origins of the French people or the Parlement of Paris—are never outlined schematically in order to define the issues and the bases of divergent opinions. Good nominalist that he is, Professor Kelley has no chapter organized around a concept, but presents his material as an interwoven series of intellectual biographies. All of these portraits, it should be noted, are accurate and useful, and some are truly gems, so that Professor Kelley, in this fashion, has provided a much needed topography of legal-historical scholarship in sixteenth-century France that will be indispensable to scholars. I am disappointed, therefore, that the book does not contain a bibliography.

JULIAN H. FRANKLIN
Columbia University

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La Politique de la Solitude: Essai Sur la Philosophie Politique de Jean-Jacques Rousseau *by Raymond Polin. Paris, Editions Sirey, 1971.—285 pp. 32 F.*

This set of extremely intelligent and beautifully written essays is an extension and, indeed, a perfecting of the Cassirer tradition of Rousseau interpretation. That is, it seeks and finds in Rousseau not only the germs of Kant's philosophy, but also of Hegel's. In this endeavor, the works chiefly considered and harmonized into a single whole are the *Second Discourse*, the "Confession of the Savoyard Vicar", from *Emile*, and the first three books of the *Social Contract*. The Rousseau who emerges is, above all, the founder of the modern philosophy of history. Here not Providence, but man ontologically free and perfectible, is the creator of his own historical fate. Thrown out of a solitary and happy natural condition by some sinister accidents, mankind is now doomed to fashion its own conditions, for better or worse. If corruption and evil have been his normal state, the "man of man," as opposed to the "man of nature," has, nevertheless, acquired new moral faculties, and it is his task to create now a society in accordance with these. Polin assumes that Rousseau's use of the word "perfectibility" is not ironic, but refers to man's potential moral growth in history. Rousseau's notion of man's potential is not as confident as Hegel's; but it points in Hegel's direction. The morality to which man is called is that of classical natural-law doctrine, and justice is its highest aspiration. This is the end to which man must be educated and this also is the basis of any moral and social order. If Rousseau was often desparate, he was not hopeless, and at any rate he recognized the duty to hope and to instruct mankind. In this we can discern the features of Kant.

In emphasizing the primacy of justice for Rousseau, Polin fully recognizes that Rousseau admitted the incompatibility of equality and justice. And, certainly, Rousseau rejected pure democracy as a hopeless chimera. But did he not profoundly regret it? In his eagerness to demonstrate the errors of Rousseau's anarchist admirers, Polin too easily glides over Rousseau's abiding and intense hatred of inequality. It was, after all, the sum of all evil for him. There is here a very sensitive account of Rousseau's discovery of the notion of alienation, of man's loss of integrity through *amour propre* and subsequent dependence on the opinions and arrangements of others. However, inequality plays a far greater part in this process than Polin allows. Rousseau's protest against the actual is here smothered by stressing his practical concern for the reintegration of man into a single solitary, but now newly moral whole, within the just society of the possible future. A corollary

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of this is Rousseau's hatred for all modern forms of government. Polin, of course, does not ignore the fact that Rousseau saw them as the primary agents of corruption, but he makes more of Rousseau's acceptance of autocratic republicanism.

One essay very successfully refutes those who see Rousseau as the precursor of totalitarianism. It concerns itself chiefly with the "general will," which Polin sees as a manifestation of the rational, public will, the precursor of what Kant was to call practical reason. As such it is the expression of man's moral autonomy and hardly an instrument of thought control. Forcing men to be free is merely bringing them to face what they know and admit to be right and true when they listen to their rational higher self. Unhappily Polin says nothing about civil religion, Spartan mores, and the educative devices urged upon the Poles. It is these that worry liberals far more than the general will. That is why Polin is unfair to Benjamin Constant, when he notes that the latter borrowed the idea of the general will from Rousseau but rejected him as an enemy of freedom. Constant had perfectly good reasons for regarding the imposition of Spartan virtues upon modern societies as a threat to individual liberty. Polin is right when he notes that Rousseau was neither a revolutionary nor a conservative, but was he really a liberal in the way that Kant obviously was? Do not all the ideological classifications constrain him unduly? What, after all, of the great Legislator? Polin makes an extraordinary suggestion about him. He considers the great Legislator a sort of Hegelian world-historical figure. Like Caesar and above all, Napoleon, the Legislator is the executor of the impersonal forces of history even though he is not aware of this function. It is not a wholly implausible notion, if one accepts the *Social Contract* as an agenda for a metaphysically necessary, if not altogether likely, prospect. However, it is difficult to believe that Rousseau gave his religious founding fathers the moral *carte blanche* which is so central an aspect of the Hegelian hero.

There are many subjects that one wishes Polin had treated more thoroughly. Among these Rousseau's religion, his revulsion against intellectuality, and his educational psychology are but a few. To say this, however, is merely to note how interesting and stimulating these essays are. They make one ask for more. The final chapter, a close and profound reading of the *Second Discourse*, stands out, and, coming at the end as it does, it arouses one's admiration for the excellence of the book's scholarship, the care and seriousness it displays on every page, and its ability to force one to rethink many familiar passages.

JUDITH N. SHKLAR
Harvard University

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Georges Mandel and the Third Republic *by John M. Sherwood. Stanford, Stanford University Press, 1970.—ix, 393 pp. $12.95.*

Georges Mandel: un Clemenciste en Gironde *by B. Favreau. Paris, A. Pedone, 1969.—iii, 295 pp. 30F.*

Georges Mandel's death has been better known than his life. Imprisoned by the Vichy regime in 1940 for attempting to keep France in the war against Hitler, he was murdered by French rightist vigilantes in 1944. He had been, some thought, the French Churchill.

These two political biographies, which improve upon the hitherto standard works of Wormser and Coblentz, shift the focus back before 1940 and to what Mandel's career reveals about the French Third Republic. At first glance, Mandel's life seems too idiosyncratic to cast much light on the regime: a Jew (born Louis Rothschild) amidst the French nationalist Right; a stiff, formal man in the "republic of pals"; a nondrinker elected repeatedly to office in the Médoc; a ferociously ambitious man who ended up one of the Third Republic's "notable failures" (Sherwood, p. 300); and a high-handed user of police power who died a martyr to the Republic. Mandel would seem merely "the most picturesque [French] politician of the interwar years" (Favreau, p. 7). Nevertheless, both authors illuminate the workings of the Third Republic.

There were two routes to power in the Republic. One might, like the Radical Herriot in 1924 or the Socialist Blum in 1936, become party leader and take office through electoral victory. Mandel chose a second route: to carve out a nonparty center position in the Chamber and become the indispensable bridge within centrist coalitions, a "ministrable."

Like many a Third Republic "ministrable," Mandel combined the offices of local mayor and national deputy. With the help of his first patron Clemenceau, he was "parachuted" into a seat near Bordeaux, where he proceeded to build an invincible party machine. The main interest of Favreau's work is his account of the mutual advantages of this arrangement. As deputy and minister, Mandel did his hometown favors, especially with postal patronage; his local beneficiaries, in turn, kept him in office despite his cutting manner and the anti-Semitic attacks leveled against him.

Far from being regarded as a conflict of interest, combined offices of mayor and deputy are at the heart of political success in the French Republics. One thinks of Herriot, mayor of Lyons for fifty years, and the current premier, Jacques Chaban-Delmas, mayor of Bordeaux. Favreau gives us only tantalizing glimpses of this phenomenon, but he touches base here with work on local-central governmental links by Jean-Pierre Worms, Pierre Grémion, and Mark Kesselman.

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This "ministrable" was almost never minister, however. Mandel also belonged to another Third Republic species: a "defeated leader," in Rudolph Binion's term. Rather than ingratiating himself with his colleagues, Mandel wanted to become the indispensable strong man. It was as Clemenceau's administrative assistant in war government (1917-18) that Mandel had first tasted political power at the age of 32. As the "tiger's cub," he had done that regime's dirty work of repressing antiwar dissent. When he finally became Minister of Posts in 1934, he set up a whirlwind of activity, breaking strikes and launching air mail and television in France. His tough manner violated prevailing political mores to the point where Mandel's career became a wait for the republic to need a strong man.

While waiting, Mandel joined those attempting to strengthen the French executive. Unlike his contemporary André Tardieu, Mandel did not want to enhance the president's powers. He wanted to create a British two-party system in France, with a powerful premier. He proposed to accomplish this by replacing the traditional two-stage elections (which encourage multiple candidacies) with winner-take-all elections. Such a system would cripple the fragmented Left; not even the Fifth Republic has dared apply it. Mandel's long fight for strong government is Sherwood's main theme, though he may have overestimated this "tortuous" man's consistency (the adjective is Sherwood's).

Mandel's call seemed to have come in 1940. As German armies poured into France, he became Minister of the Interior, with the French police in his hands again. Mandel's conservative allies regarded this war as "Stalin's War," however, and they had no use for another Clemencist tiger. At the moment of the Armistice, Mandel was isolated and alone.

ROBERT O. PAXTON
Columbia University

The New Order and the French Economy *by Alan S. Milward. Oxford, Clarendon Press, 1970.—viii, 320 pp. $12.00.*

Relying primarily upon German documents, the author aims at discovering the extent of Nazi exploitation of the French economy and seeks to interpret the nature of an occupied economy. Accordingly, the book is organized into two chapters on German goals, one chapter on the machinery, and three chapters on the overall extent and methods of exploitation. Special chapters on coal, iron ore, bauxite, wolfram, and agriculture conclude the investigation. Alsace-Lorraine as well as Nord and Pas-de-Calais are largely excluded, and the deliveries from the

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French colonies are mentioned only occasionally..

Measuring the extent of the exploitation turned out to be quite difficult. Relevant French documents were either not available because of the fifty-year secrecy rule, or the claims of the reparations commission were often inflated. German agencies were more interested in increasing the volume of the delivered goods than in carefully recording their value. Many goods were traded outside the market nexus because delivery was separated from payment and a significant portion was paid for by the French government in the form of costs of occupation or clearing accounts. Fairly reliable figures are thus available only for these centralized payments. The record of the many other extortions could only be described, not measured in values. The author concludes "that the general level of exploitation must have been between 40 per cent and 50 per cent of French capacity in autumn of 1943" (p. 136). But the postwar calculations that the French share in the German Gross National Product rose from 2.6 per cent in 1940 to 8.2 per cent in 1943 (p. 272) are regarded only as vague approximations.

While the factual investigation is competently done, limited mainly by the inadequacy of available information, the same cannot be said for the efforts to interpret either the occupied economy or the Nazi empire. Received economic theory usually takes the national economy as its framework and presupposes voluntary exchange between individuals among countries. In order to come to grips with the occupying power as a motivator of economic activity, the author examines the role of conquest and exploitation in transnational transactions. A doctrinal survey distinguishes between liberal and fascist theories of conquest and identifies politically created profit opportunities as the rationale for the subsequent policies of exploitation by the occupier. Unfortunately, the survey is incomplete, does not include Hitler's own theory of racial imperialism, and fails to specify the nature of the occupying power. Nevertheless, the thesis of exploitation serves as a useful organizing principle and gives direction to the best chapters of the book.

When it comes to the Nazi empire, the author experiments with the "New Order." But his chapter two fails to identify the nature of such an order because it overlooks the fact that the Nazis had used this concept to camouflage their imperialist intent and instructed their press in November of 1940 to drop the "New Order" completely (cf. Paul Kluke, "Nationalsozialistische Europaideologie," *Vierteljahrshefte für Zeitgeschichte*, July 1955, p. 259). In the description of Speer's effort to shift to production of consumer goods in occupied countries, the New Order becomes an explanatory principle. Sauckel's policy of forcefully recruiting French workers for Germany exemplifies the Nazi version of

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the new order, while Speer's policy is interpreted as "an attack on the National Socialist ideas of the New Order" (p. 146). Speer's claim to "a unified tariff zone" in his interrogations at Nuremberg are taken at face value and are placed in between the "work of Rudolf Delbrück and the Common Market" (p. 148). Speer's real aim of 1943 "to make the total production capacity of the occupied western territories available to the greatest possible extent to the German war economy" is quoted (p. 164), but given no weight. The relevant documents support only the conclusion that Sauckel's and Speer's policies constitute different methods for the same Nazi policy of exploitation, which were both sanctioned by Hitler. The myth of the New Order led to a kind of economic "ism" that foiled the significant attempt to write economic history in terms of economic systems.

ARTHUR SCHWEITZER
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The Vanishing Peasant: Innovation and Change in French Agriculture *by Henri Mendras, translated by Jean Lerner. Cambridge, Mass., The M.I.T. Press, 1971.—xiv, 289 pp. $8.95.*

Pont-de-Montvert: Social Structure and Politics in a French Village, 1700-1914 *by Patrice L. R. Higonnet. Cambridge, Mass., Harvard University Press, 1971.—xiv, 217 pp. $8.50.*

"One or two billion peasants at the threshold of industrial civilization: this is the major world problem facing the social sciences in this second half of the twentieth century" (p. 1); by studying the contemporary French peasantry, now making the transition to industrial civilization—and therefore "vanishing"—Henri Mendras hopes to illuminate the problems of transition common to all peasant societies. The first section of the book is a rather lengthy theoretical analysis of the French peasant's traditional economic, social, and cultural system and how it is undermined by new technical and economic developments. The remainder is a discussion of peasant attitudes toward changes in agriculture and rural society, based largely on survey data collected by Mendras and his collaborators. The final chapter consists of some astute and interesting speculations on the shape of French rural society in the future. The book contains valuable insights into the peculiarities of peasant society and convincing demonstrations of the need to modify sociological and economic concepts based on urban and industrial experience when analyzing the peasantry. Furthermore, Mendras uses the survey data effectively to point out the contradictory nature of peasant attitudes

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during the transition from "traditional" to "industrial" logic.

But for all the valuable information and insights, Mendras does not succeed in his larger purpose. His most important failing is a foreshortened historical perspective, which leads him to a highly stereotyped and romanticized conception of what the French peasant and French rural society were like in the period before World War II and which causes him to overestimate the novelty and suddenness of changes taking place in the countryside since the war. According to Mendras, the present generation of peasants was formed in a "traditional," "autarkic," "face to face" society, in which "subsistence polyculture" was the rule, in which a man's status was determined by his birth, in which his horizons did not extend far beyond his fields, and which, being in equilibrium, resisted all efforts at innovation. Thus, in a single generation, the French peasant has been thrust from "a routine of delightful immobility" into a violent and exhausting movement to which he must unreservedly give his all, with unpredictable outcome" (p. 127). This ignores the immense if often gradual changes that had taken place in French rural life between the eighteenth century and the Second World War—integration into the national market, improvement of communication, nationalization of political life, the spread of universal primary education, improvements in agricultural techniques and crop yields, and spectacular increases in standards of living. The problems facing the contemporary French peasant are simply incommensurate with those facing present-day Asian and African peasants. Unless this is clearly recognized, the study of Western rural people can shed only the most refracted light on problems of agriculture in the Third World.

Professor Higonnet's book provides a useful antidote to the ahistorical assumptions of Mendras. Pont-de-Montvert is a small Protestant village of about a thousand souls in the harsh, remote, and infertile Cevennes mountains of south-central France. By tracing the experience of this single village through a period of more than two centuries, Higonnet provides a valuable example of one rural community undergoing a long and gradual—but far-reaching—process of modernization. From the seventeenth century to the early nineteenth century, Pont-de-Montvert was an impoverished but highly stratified society. While there was some shifting in the families that made up the different classes within the hierarchy, the structure itself remained essentially unchanged until the nineteenth century, when integration into the national market brought about emigration of both the very poor and the notables, together with an increase in wages, a fall in the price and relative value of land, the spread of primary education, and a general rise in living standards. The result of all these changes was what Higonnet calls the "breakup of the social structure"—"leveling" might have been a better

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word—and the decline of the village as an autonomous and vital community. The case of Pont-de-Montvert indicates how thorough-going were the changes in French rural life in the nineteenth century, even though the peasant, as defined by Mendras, did not vanish.

But while Higonnet's research demonstrates both the value of detailed historical investigation into problems of social change and the abundance of data available for such investigations, the structure of his argument is shaky. Although his evidence points to the nineteenth century as the period of crucial social change, his analysis of this period is sketchy and impressionistic, outlining the results of the changes but giving us no sense of the process. Instead, he devotes most of his energy to the seventeenth and eighteenth centuries, attempting above all to elucidate the relationship between social structure and politics in two great revolutionary upheavals—the Camisard Wars of 1702-04 (a Protestant revolt finally put down by royal troops) and the French Revolution of 1789. Higonnet considers it paradoxical that the first of these upheavals produced a fairly clear class alignment while the second did not —in spite of the fact that economic divisions between the rich and the poor had, if anything, increased in the interval. While Higonnet turns up some interesting material in his efforts at resolving this paradox, the patterns of political behavior he is attempting to explain seem far from paradoxical. In the Camisard Wars the lower classes revolted against the persecuting state while the equally Protestant upper classes, caught between their distrust of the Catholic state and their abhorrence of disorder, generally collaborated reluctantly with the state. But when the Third Estate seized power in Paris in 1789, the situation was entirely different. In accepting a revolution against the Catholic monarchy, the upper class of Pont-de-Montvert was also supporting the existing government; their hatred of the Catholic kings and their love of order did not conflict. They therefore found themselves in political agreement with the lower classes in 1789. Higonnet's argument that the upper classes had reestablished control over the lower classes by reasserting their leadership in Protestant religious life seems a little beside the point. But if the framework of Higonnet's argument is awkward, the materials he assembles in constructing it are interesting enough to make this a useful book. One last objection, however: in a book based largely on quantitative evidence, it is perverse to include so few tables. This makes it impossible for the reader to draw his own conclusions from the data, and it also detracts from the rigor and precision of the argument.

WILLIAM H. SEWELL JR.
University of Chicago

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The French Democratic Left, 1963-1969: Toward a Modern Party System *by Frank L. Wilson. Stanford, Stanford University Press, 1971.—ix, 258 pp. $8.95.*

In 1963, the French democratic Left included two traditional parties, the Socialists and the Radicals; a New Left group, the PSU; and an amorphous collection of clubs organizing "amateur Democrats," in the hope of revivifying the movement through rethinking its attitudes and exploiting the presidential campaign of 1965. Using that campaign, two politicians attempted to reconstruct the movement: Gaston Defferre by bringing MRP (the Christian Democratic party) into a federation of the Left and, after his failure, François Mitterrand by federating only the traditional Left forces. Mitterrand succeeded in unifying the electoral and parliamentary efforts of the Left, but only until the election defeat of 1968 and the discrediting of the Communist alliance (with which he was identified) after the Czech invasion. With Defferre's calamitous failure in the 1969 presidential election, the democratic Left found itself weaker and more demoralized than ever before.

Mr. Wilson's account of these events is thorough, lucid, informative, and intelligent and is accompanied by some sensible generalizations about the process of party reform, stressing the political as against social and economic factors. I accept most of his analysis—the role of the clubs (on which he is particularly good); the traditional Left's failure to adapt; the strength and weakness of Defferre's campaign; the difference between Defferre's approach and Mitterrand's; the influence of local politics; the problem of the Commuinst party. He is almost always accurate (but Defferre supported de Gaulle in September 1958, and the Communists opposed the federation's censure on foreign policy in 1966, contrary to the impression given on pp. 113 and 162n). Yet in some ways his book is rather unsatisfactory. Mr. Wilson is a firm believer in the inevitability and (therefore?) desirability of the "catch-all party," without ideology or class identification, aiming at an electoral majority. This seems to make him more indulgent to the Gaullists, who have such a party, than to the Left, who (apart from Defferre) have not tried to be one. He thus excuses the Gaullists for trying to keep out at all costs the federation and the Communists as antisystem parties—without noting that they were just as hostile to Defferre who accepted the new institutional rules. He swallows their exaggerated claims to be progressive and reforming—even in social legislation where, quite forgetting the record of 1956 he says, "the Left talked of reforms and did nothing." More seriously, he blames disillusionment with the Socialist party primarily on its detested sin of ideology, putting no stress on its performance. Yet

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after winning the 1956 election on a peace platform, the Socialists and Radicals, rising above principle, took responsibility for the Algerian War, which they could neither win nor settle. That (following Indochina where MRP had done the same) was the problem which destroyed the Fourth Republic. Lumbered with men like the Socialists Robert Lacoste with his unlucky reference to the "last quarter of an hour" in Algeria in 1957, or Max Lejeune, with his heavy responsibility for covering the excesses of repression, and like André Marie, a Radical ex-premier whose long record of conservatism and equivocation was well worthy of a senator from the Deep South, the Left could hardly pretend to credibility as a progressive force; yet it is not these men but their critics who attract Mr. Wilson's censure, for the three politicians "had proven their ability to win votes from a broad segment of society" (p. 150). Writing in 1969, an American author might have been expected to show more sensitivity to the critics' reasons for political disillusionment—perhaps even to feel a little less superior to the people he is writing about.

P. M. WILLIAMS
Nuffield College, Oxford

Politique belge '68: Les évenements, les hommes, les attitudes politiques *by the Institut Belge de Science Politique. Brussels, the Institute, 1970.—409 pp. 290 FB.*

Le Comportement politique des électeurs belges *by Nicole Delruelle, René Evalenko, and William Fraeys. Brussels, Institut de Sociologie, Université Libre de Bruxelles, 1970.—265 pp. 360 FB.*

The politics of the smaller European democracies have, happily, been getting some of the attention which political science used to reserve for the larger European states. The two books under review are of interest, not only for their information about a political system of intriguing changes and continuities, but even more—for most readers of this *Quarterly*—for the materials they offer for comparative work and at least middle-range theories of democratic politics. Space here permits only a listing of some of the books' contents, rather than a report of findings or an analysis of methods and results.

Politique belge '68, like the French *Année politique*, offers chronology and documents, but it is mostly a collection of topical essays. Pride of place goes to the politics of language and regional conflicts, which broughr down the Vanden Boeynants government and caused the dissolution of

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Parliament. The old cleavages and some new issues and alignments figure in Wilfried Dewachter's analysis of the elections and Mieke Claeys-Van Haegendoren's account of the long cabinet crisis between the elections and the formation of the Christian Social-Socialist government headed by Eyskens. Students of coalition practice and theory will find the electoral tactics of the regional-linguistic groups within the nominally still "national" parties, and the later negotiations within the parties and among the parties, of great interest.

The Vanden Boeynants government had fallen because of its failure to recognize the force of the Flemish demand for the expulsion of the French-language section of the international Catholic University from its ancient seat in the Flemish city of Louvain (Leuven). André Philippart's article explains the politicization of the issues of university expansion, organization, and location. Micheline Crêteur's account of university students' movements shows the interesting combination of realism with radicalism in the Flemish students' fight for an exclusively Flemish university at Louvain. M. P. Herremans analyzes "language and community problems" in the immediate detail of 1968 and their longer-run setting. "Community problems" has become the official term for the mixture of language and regional problems, separate but overlapping. They are not always well sorted out in political debate, as the definition of the units of analysis and action is itself one of the issues of controversy.

One essay looks beyond the Belgian scene. Jules Gérard-Libois discusses the limitations (many and deep) and the possibilities (few, but interesting) of independent action or initiatives in the foreign policy of a small member of the Western bloc. He also recalls the constraints upon national policy, and especially upon parliamentary control of the executive, which flow from the nation's international commitments.

The elections dealt with in *Politique belge '68* were a confirming election, following on the shocks to an old party system produced by the municipal elections of 1964 and especially the parliamentary elections of 1965. The authors of *Le Comportement politique* . . . carried out an extensive survey of voter opinions just after the 1968 elections, with questions going back to voting behavior in 1961 and 1965. Their work abounds in fascinating (if repetitiously arranged) detail and suggestive hypotheses. The aging of the Socialist electorate and the appeal to young voters by the regional and linguistic parties (especially the Flemish People's Union, the Volksunie) come through sharply. At the usual dismal level of voter acquaintance with political issues and leaders, women trail men by further dismal lengths. Subjective identification with social class appears as an essential intervening variable between socio-occupational

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status and political behavior, although it is a variable itself calling for explanation. Despite a certain "deconfessionalization" of the parties, religious practice or its absence continues to play an essential role in voters' identification with the two major old parties, the Christian Socials and the Socialists. The third national party, the Party of Liberty and Progress (successor in 1961 to the old Liberal party, minus its anti-clericalism) relies chiefly on its identification as a party of the bourgeoisie.

The authors suggest a typology of Belgian voters, inspired in great part by the work of Klaus Liepelt on German and Austrian voters. Liepelt of course had little need to worry about linguistic and regional categories of self-identification. But following him in Belgium leads the authors to obliterate entirely the distinctiveness of those parties whose successes cause them to speak, appropriately, of a "rupture" in the party system in 1965. They lose the distinctiveness of the strongest of the "community parties," the Volksunie, by placing it first with the "political family" traditionally attached to Catholicism, and then with the political family attached to the bourgeoisie and the lower middle classes. This treatment not only loses the *Volksunie* as an entity, but also disregards the aims, program, and political style which are the party's reasons for being. Similarly, the authors drown the two French-speaking "community parties," the *Front des Francophones* (Brussels) and the *Rassemblement Wallon,* in the bourgeois and lower middle-class political family.

The readiness to place a party with a high proportion of Catholic voters in the Catholic political family results in another confusing ascription. The historical summary chapter classifies the Union Démocratique Belge as a "Catholic dissidence," although the whole reason for its formation in 1946 was to shake Belgian politics loose from its religious-ideological moorings and to create a nonconfessional party like British Labour. Political taxonomy should at least place a more accurate tombstone on a bold venture, for which Belgian politics was not ready until the three traditional parties had reached the truce of 1958 on the school question.

Proliferating in market research *à l'Américaine,* Belgium has until recently had little survey research of disinterested sponsorship or academic quality on party preferences and voting behavior. Perhaps this is why the able authors of *Le Comportement . . .* are so uncritically enthusiastic, not only about the "precision of pre-electoral surveys" (they must have been writing before the British election of 1970), but also about the explanatory power of opinion surveys in political sociology.

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All of us, however, must hope for the continuation and expansion of political history, analysis, and survey research in a country whose experience can contribute so much to the data and theory of conflict and compromise in industrial nations, and particularly in religiously and linguistically divided societies.

VAL R. LORWIN
University of Oregon

Walter Rathenau and the Weimar Republic: The Politics of Reparations *by David Felix. Baltimore, The Johns Hopkins Press, 1971.—xii, 210 pp. $9.00.*

This slim volume does scant justice to a man who—through his prolific writings on political and intellectual subjects, his eminence as a leading industrialist, and his distinguished services to his country during World War I and under the Weimar Republic—became one of the most prominent, most admired, and most derided men of his period—until he was finally assassinated by right-wing extremists in June 1922. The author maintains that "Rathenau was a distinguished ambiguity at the age of 50" and that his "books were completely lacking in original ideas, self-indulgent, pretentious in style, and disdainful of proof as they proceeded from one arbitrary statement to the next" (pp. 47, 50). But such sweeping statements hardly do justice to Rathenau as a writer and a politician, nor do they take into account the enormous influence which he exercized, not least on his political enemies. Dr. Felix is on firmer ground when he criticizes, in scathing terms, the folly of Allied reparations policies which attempted to extract from the defeated enemy sums that Germany was unable to pay without courting economic collapse—policies likely to promote communist revolution, which the Allies aimed at avoiding. In the end, Germany paid very little. As the author puts it: "By and large American and Allied creditors and investors paid German reparations. It was only reasonable: Germany did not have the money and they had" (p. 184). Yet, as he also demonstrates, the issue time and again played into the hands of the bitter enemies of the Weimar Republic—another result that was certainly not in the Allies' interest.

While it is refreshing to have these points brought out so forcefully and provocatively, it is regrettable that Dr. Felix has such limited knowledge of the essential background to the reparations issue. Thus he states equally forcefully: "The early history of the Weimar Republic demonstrated to the satisfaction of the Allies, however, that the German

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military forces, still much greater than foreseen in the treaty, could not keep internal order" (p. 10). But "law and order" were preserved by them only too successfully, from the days of the Spartacus Rising to those of the Kapp Putsch, crushing time and again alleged or real risings of the extreme Left by brute force. One of the units used for this purpose was the Ehrhardt Brigade, which after its dissolution by the authorities became a semi-underground force, the Organisation Consul, which organized political murder, among others that of Rathenau. But it never was "a mysterious military office functioning as a kind of general staff for all the Free Corps," nor was it ever a "secret military office which had been useful to [Chancellor] Wirth in the defense of Upper Silesia," as we are informed on pages 101 and 126. It is a pity that a serious study of an important subject should be marred by such misstatements, which could have been rectified so easily. Greater attention to fact, rather than provocative statements, would have been welcome.

F. L. CARSTEN
University of London

Imperialism and Chinese Nationalism: Germany in Shantung *by John E. Schrecker. Cambridge, Mass., Harvard University Press, 1971.—xi, 322 pp. $12.00.*

In the last decade of the nineteenth century, Alfred Tirpitz, a master politician as well as a naval strategist, maneuvered a hesitant German public into supporting a massive naval armament program. Writing in the *New Republic* forty years ago ("Making a Bigger and Better Navy," Oct. 14, 1931), Charles Beard remarked that Mahan's works had gone to Tirpitz's head and to the Kaiser's as well. With *The Influence of Sea Power upon History* as "a kind of marine bible," Tirpitz, who became naval minister in 1897, tried to compete with Britain by using the German navy to build an overseas commercial empire.

One of the results of this program was the German adventure in Shantung, which is the subject of Mr. Schrecker's absorbing study. Utilizing Chinese and German archives, Schrecker skillfully traces the failure of the Tirpitz doctrine in China. The German Navy seized Kiaochow in 1897, and during the following decade Germany poured money and the fruits of her vaunted technology into the leasehold and its hinterland. Yet by 1907, the German threat was contained. Germany's sphere of influence in Shantung no longer existed, and, more important, her meager commercial and industrial profits were hardly commensurate with her huge investment. Why did Germany fail?

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On the international scene, she was frightened by the Anglo-Japanese Alliance of 1902 and the Japanese defeat of Russia in 1905. The masterminds of the imperialist policy, including Tirpitz himself, saw the danger of Germany becoming completely isolated, or even ejected from other parts of China, especially those under British control, if she continued to pursue a forward policy in Shantung. Mumm, her ambassador to China, warned that a German escalation in Shantung would make it necessary to fight Japan and/or Britain: "The more Germany arms Kiaochow, the more she becomes obligated to defend it militarily."

Commercially, Germany failed because the Chinese and Japanese were in a better position to reap the benefits of German-sponsored development. Even modern German equipment, as applied to mining, was of no avail since the Chinese, using traditional but cheaper methods, did a better job of mining coal. The Chinese also proved adept at learning modern techniques that the Germans introduced. Thus German businessmen, including the superpatriot, Krupp, were reluctant to risk their capital in Shantung. But German enterprise did not fail completely: German beer sold very well.

Yet ultimately, what really shattered the German dream in Shantung was the rise of Chinese nationalism after 1900. It is in expounding upon this theme that Schrecker makes a brilliant and highly original contribution. He shows how a bankrupt and militarily weak Chinese regime was nevertheless capable of exerting enough pressure—both politically and economically—to defeat German imperialist aims. The German ambassador was quick to realize that China was no soft touch. In 1905, Mumm reported to Berlin that "the wonderfully dumb Chinese have finally become clever enough to realize that we can't stay permanently in a place to which we have no right."

Schrecker has not only written the definitive study of Germany's failure, but he has opened an entirely new approach to our understanding of the clash between European imperialism and Chinese nationalism. His book should prove indispensable to scholars in both fields.

HAROLD Z. SCHIFFRIN
The Hebrew University of Jerusalem

Icon and Swastika: The Russian Orthodox Church under Nazi and Soviet Control *by Harvey Fireside. Cambridge, Mass., Harvard University Press, 1971.—xx, 242 pp. $8.00.*

The occupation of parts of the Soviet Union by German forces during World War II provided an opportunity for new responses by the people in

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the occupied area, posed challenges for the Soviet regime, and led to the creation of new types of sources in the form of German records and postwar interviews with Russian refugees. Professor Fireside has used these sources, along with published works, to probe the fate of the Russian Orthodox Church. He reviews the successive persecutions of the church before 1939, examines the shifting policies and practices of the Germans, assesses the reaction of the clergy and general population under occupation, and traces the major change in Soviet church policy of 1943.

The book is a useful introduction to an important topic but is not the study it could have been because of an error of geographical focus. Of the pre-1939 Soviet territory that came under German occupation, the major portion was under military administration, with the civilian administration controlling mainly areas annexed to the Soviet Union in 1939 and 1940. In the face of this, the author concentrates on the area under civilian administration and often leaves the reader thinking that he is dealing with the same people whose fate under Soviet rule from 1917-39 was previously discussed, when in reality a large proportion—probably a majority—were not then in the Soviet Union. A map showing pre-1939 boundaries and wartime districts would have revealed this incongruity; it would also have turned the author to the appropriate records. These are the files of the German Army Group Rear Areas (*Heeresgebiete*), Army Rear Areas (*Korück*), and security divisions, none of which appear in the text or notes. (A map would also have shown the large piece of pre-1939 Soviet territory under Rumanian occupation.)

The result of the misfocus in Fireside's research is that there is extensive discussion of Alfred Rosenberg's unapplied visions and of controversies in the areas only briefly incorporated into the Soviet Union, while the military rulers of great stretches of pre-1939 Russia appear for a few pages (pp. 117-23) under the quaint rubric of "ancillary agencies," discussed on the basis of various odds and ends—but not their own records. On the few occasions when his theme takes the author into the area under military administration, such as the role of Sergius the Younger in the Pskov region (pp. 133-36), the peculiarity of this approach becomes grotesque. It should be noted, however, that when Fireside does deal with the one large area that was both in the pre-1939 Soviet Union and under civilian administration, the central Ukraine, his account is perceptive indeed. Reviewers are well cautioned against reproaching an author for not writing a different book, but in this case the author thinks he has already written it. The subject is significant; Fireside has both interesting ideas and a thoughtfully original approach. A thorough look

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at the subject would be of great value and would also provide a clearer background for an analysis of the Soviet shift in religious policy, surely the major phenomenon dealt with by this book.

GERHARD L. WEINBERG
University of Michigan

The Commissariat of Enlightenment: Soviet Organization of Education and the Arts under Lunacharsky, October 1917-1921 *by Sheila Fitzpatrick. Cambridge, Cambridge University Press, 1971.—xxii, 380 pp. $13.50.*

The Russian Revolution is known to most Westerners as a political and cultural upheaval of unprecedented violence and thoroughness, a cataclysm in which not only European political traditions but cultural and educational ones as well are rejected by the leaders of the emerging Soviet state. Against the background of this common viewpoint it may be surprising to learn that during the first twelve years of the Soviet government the official in charge of the arts and education was a thoroughly Westernized philosopher-poet, a playwright and lover of the arts, A. V. Lunacharsky, a man who in Naples once lectured so movingly on the Renaissance that all his listeners were reportedly brought to tears.

Lunacharsky successfully defended Russia's cultural heritage against both the most radical artists, who discounted all art except their own, and the most militant politicians, who saw all art as an intolerable luxury in a time of revolution. Under his administration, the most influential educational theorists were probably John Dewey, Maria Montessori, and Leo Tolstoy, rather than explicit Marxists. In contrast to the fate of the Académie des Sciences of Paris, which was destroyed in the French Revolution, the old St. Petersburg Academy of Sciences continued its work under the Soviet regime, eventually becoming the center of theoretical science throughout the Soviet Union. In sum, the early years of Soviet Russia show a pattern in the cultural, educational, and scientific spheres thoroughly recognizable to a West European or an American. We now have a monograph which probes this interesting period with both scholarship and grace.

Sheila Fitzpatrick originally planned her study as a biography of Lunacharsky, the central figure. While doing research on Lunacharsky in Moscow, she was granted access to the archives of the People's Commissariat of Education and thus became one of the very few non-Soviet

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scholars to be allowed to examine post-Revolution archives. She recognized the opportunity to make a case-study of the problems of revolutionary government and has produced, instead of a biography, a history of the creation of a bureaucracy and an analysis of the ways in which that bureaucracy coped with a whole series of problems: the liquidation of illiteracy and the spread of "enlightenment"; the elaboration of a new philosophy of education and the struggle over its principles; the analysis of the role of the arts in a revolutionary situation and the administration of an existing network of cultural institutions; and the charting of the future of technical and higher education.

One of the highlights of the book is the debate between Lunacharsky and the economist E. A. Preobrazhensky; the economist believed that Lunacharsky was wasting hard-earned resources on cultural luxuries, producing enlightenment when people needed bread. Preobrazhensky maintained:

> It must be remembered that enlightenment exists on the basis of a definite surplus product of society. Where that product is small, then the 'fruits of enlightenment' will inevitably also be small. It is improper, above all for our socialist state, to imitate the ruined aristocrat who refuses to mend the roof so as to buy a valuable painting or an expensive library.

And Lunacharsky answered:

> If, for example, we came to the conclusion that it was necessary to sell our whole museum collection abroad, exchanging it for the appropriate quantity of flour, or to stop all artistic education or all artistic life in the country completely, we would hardly be acting rationally . . . .
>
> Because joyless life, deprived of all culture, . . . is a hard-labour sentence . . . . the question is, are even poor and hungry people to have the great comfort of art or are they not?

Perhaps these quotations will serve to show that Fitzpatrick's excellent book in its evocation of personal qualities still bears signs of the biography it was originally intended to be. Professor Fitzpatrick has not exhausted her topic, and there are moments when the social and political background is rather thin, but her book is one of the few recent works in Soviet history which have both a solid, scholarly foundation and a sensitive, appealing exposition.

LOREN R. GRAHAM
Columbia University

The Quest for National Efficiency: A Study in British Politics and Political Thought, 1899-1914 *by G. R. Searle. Berkeley, University of California Press, 1971.—x, 286 pp. $8.50.*

In the closing years of the nineteenth century, many British commen-

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tators became worried about their country's loss of position economically, relative to such rising powers as Germany and the United States. With the Boer War and its revelations of incompetence and, in Searle's words (p. 50), "organizational weaknesses, not only in the army, but in the whole machinery of government," a sizable and diverse group of people took up the cry of "national efficiency" and strove to make it the basis for a sweeping, if not very sharply defined, renewal of British institutions.

The author admits the looseness of both the membership of the "efficiency group" and their ideas. Ranging from Sidney and Beatrice Webb through Lord Rosebery to (for some purposes at least) A. J. Balfour, they came from all parties and none. At times they tried to work through the established political machinery, but many of them showed a persistent longing for a national coalition that would rise above party to give the nation the thorough overhaul of its institutions they thought it needed. From one standpoint, the movement failed because it would not accommodate itself to the great parties; its weakness in this respect was symbolized by the strange, erratic, and, in the end, wholly futile wanderings in the political wilderness of its most glamorous proponent, Lord Rosebery.

Skepticism about party politics aside, the substantive components of the movement ranged from a vague desire to see more businessmen and business methods in Whitehall to the discovery that in such fields as health and education Britain was lagging behind other advanced countries in ways that made it difficult to compete on even terms, let alone regain the supremacy of the mid-nineteenth century. There was considerable confusion and not a little cant in the "efficiency group." Searle is clear that "the loss of markets to the more systematic Germans and Americans suggested that before British businessmen could come to the rescue of the state, they first needed to put their *own* house in order" (p. 92), but no one seems to have sensed this irony at the time.

Concrete achievements deriving from this movement are few and not always easy to document. Searle does not make excessive claims for it, unless one might so describe his straining somewhat to see Lloyd George's notorious flirtation with a coalition government in 1910 as a manifestation of the drive for national efficiency. His "three case studies" in the final chapter are the Education Act of 1902, the Committee of Imperial Defence, and the minority report of the Poor Law Commission. All owed something to the quest for national efficiency, but obviously a variety of other factors played a part in each. Searle makes a convincing case for the view that the Webbs overreached themselves in the minority report, aspiring to a degree of social engineering and manipulation of people through (in the words of the report quoted

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on p. 242) " 'Human sorting houses' " that alienated—and with good reason—Liberals and Conservatives of all but the most extreme variety.

It is difficult in a study of this kind to avoid the twin dangers of allowing the subject to appear as a potpourri of ill-conceived and unrelated notions, or imposing upon it through hindsight a disciplined coherence which in reality it lacked. Searle's straightforward, careful, and perceptive account is reasonably successful in avoiding these pitfalls. What is most thought provoking is the realization that the theme of "national efficiency" has never wholly vanished from British public life in this century, yet the goal appears almost as elusive in 1972 as it was when Rosebery was proclaiming it seventy years ago.

RICHARD W. LYMAN
Stanford University

Church and State in Modern Ireland, 1923-1970 *by J. H. Whyte. New York, Barnes and Noble, Inc., 1971.—xii, 466 pp. $13.50.*

In his *Church and State in Modern Ireland, 1923-1970,* Dr. J. H. Whyte has set himself three tasks. The first is to give an account of the relations between church and state since 1923, and the second is to provide a detailed examination of what was, in effect, the *cause célèbre* of those relations in the famous mother and child causes of the various Health Bills and Health Acts between 1947 and 1953. Dr. Whyte's third task is to answer the question, "How much influence does the Catholic hierarchy wield in Irish politics?" In coming to terms with his first two tasks, which are not really differentiated in kind but in degree, Dr. Whyte has structured a dialogue between church and state in Ireland that is a model as far as sane and sober judgments are concerned. His rendition is all the more remarkable because the task is complicated not only by the passions and tensions inherent in the subject, but by the even more delicate problem that most of the partisans are still alive and were interviewed by him.

The story is told chronologically, and the book is divided into three parts. In the first (1923-44), Dr. Whyte presents a dialectic on the effects produced by the enshrining of a Catholic moral code in the law of the state (1923-37) and by the Catholic social movement becoming a force in that state (1931-44). The effective result of this dialectic was a synthesis (1944-46) in which the main issue of the second period (1944-59), vocationalism versus bureaucracy, takes shape and is introduced. When the first Health Act was passed in 1947 the stage was finally set for the crisis of the book—the denunciation by the Irish bishops as a body of the Health Bill proposed by Dr. Noel Browne, minister

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for health in the interparty government in 1951. This crisis was finally resolved in 1953 by De Valera's *Fianna Fail* party which brought in a bill acceptable to the bishops as far as Catholic social teaching was concerned. Though the bishops essentially had their way in this extended crisis, the result in time was a stiffening resistance, from 1953 to 1959, to old attitudes about the relations between church and state. The third and final period (1959-70) discussed by Dr. Whyte is not yet over, but it has been marked by a spirit of accelerating change, especially after 1966, in which the old attitudes have been eroded. The lesson in the narrative, if I read Dr. Whyte correctly, is that the difficulties between the government and bishops has had more to do with misunderstandings and breakdowns in communications than with ill will or an unscrupulous abuse of power by either side. Still the crucial question remains—why these misunderstandings and breakdowns in communications? Certainly much can be attributed, as Dr. Whyte rightly points out, to the personalities involved as well as to outmoded and inadequate machinery, but perhaps even more can be attributed to the historical frame which constrained the personalities to act in the way that they did.

Finally, in his conclusion Dr. Whyte turns more explicitly to the third of the tasks he set for himself by attempting to evaluate how much influence the bishops wield in Irish politics. He points out that Ireland is neither the theocratic state that some anticlericals would have us believe, nor is the church simply just another interest group exercising its rights in a pluralistic society, as some clerical apologists are wont to assert. The answer lies somewhere between these two extremes, but precisely where it is hard to say. Dr. Whyte concludes, in fact, that "no simple answer is possible: it depends on circumstances." But is it not possible to generalize about the influence of the Irish bishops in politics? Surely on the basis of the evidence cited by Dr. Whyte, it can be argued that the high watermark of the bishops' influence was reached in the promulgation of the Constitution in 1937 and began to recede after that date. Before 1937, the several Irish governments, attempting to consolidate a revolution recently made against the British, could not afford a quarrel with the most important and powerful vested interest in the country, while since then, the bishops' influence has been more apparent than real because the consequence of any exercise of their power has been a continuing diminution of their influence. The historical lesson to be learned from their apparent victory in 1951 in objecting to the mother and child scheme is that their influence has never been the same, and no one is more aware or wary of this than the bishops as a body.

EMMET LARKIN
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Governing Without Consensus: An Irish Perspective *by Richard Rose. Boston, Beacon Press, 1971.—567 pp. $12.50.*

Ireland's English Question: Anglo-Irish Relations, *1534-1970 by Patrick O'Farrell. New York, Schocken Books, 1971.—viii, 336 pp. $11.50.*

"It is a mere statistical inquiry, and proceeds very satisfactorily, but, as you say, proves only what everybody knows," wrote George Cornewall Lewis to a friend in 1834 concerning his own work on an early piece of socioreligious research in Ireland. "Nevertheless," he continued, "it is something to establish disagreeable truths beyond the power of contradiction." The disagreeable truth was that the established, Protestant-Episcopal "Church of Ireland," after nearly three centuries of legal privilege, still enjoyed the adherence of only a small fraction of the population. One is tempted to make a similar comment about Richard Rose's *Governing Without Consensus*, which establishes incontrovertibly two disagreeable truths: that the Northern Ireland regime has failed to win the allegiance of its Catholic minority and that the allegiance of a substantial number of Protestants is of a very conditional sort. Based on a careful survey of the attitudes of 757 Protestants and 534 Catholics in 1968, it is the most comprehensive study of its kind for Northern Ireland.

Rose goes beyond "mere statistical inquiry," however, to develop a general conceptual framework for describing the authority of a regime. He defines two characteristics—*support* for the regime and *compliance* with its basic political laws—which he uses to establish four ideal types of regime. The "fully legitimate" regime enjoys high support and high compliance, the "coercive" regime low support but high compliance, the "repudiated" regime (e.g., the Third Reich in Germany today) low support and low compliance, and the "isolated" regime (e.g., an African country with communications too poor to secure compliance with all its laws) high support but low compliance. To these categories there correspond four types of individual outlook: the fully allegiant citizen, the repressed individual, the rebel, and the "ultra," who "supports a particular definition of the existing regime so strongly that he is willing to break laws, or even to take up arms, to recall it to its 'true' way." Northern Ireland is classified as a "divided" regime because, according to Rose's survey, a significant share of the population falls into each of the four categories. Only 25 per cent are fully allegiant individuals; 7 per cent are "repressed," willing to comply with the regime's laws but withholding support; 11 per cent take attitudes characteristic of the rebel; and a striking 26 per cent are ultras. Another 31 per cent are uncertain in their support, compliance, or both. Rose's analysis is no doubt of great theo-

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retical interest for understanding political systems which tend more strongly to one of the four ideal types and provides considerable grist for the mills of current discussion of Northern Ireland as well.

Patrick O'Farrell has produced a survey of Irish history since Tudor times "given to the argument that religion was the fundamental barrier to harmonious Anglo-Irish relations." The reader who is unfamiliar with Irish historiography might wonder whether this, too, is not "what everybody knows," but in fact the book is a minor landmark in Irish historical writing. About 1938, with the founding of *Irish Historical Studies*, Irish historians, North and South, deliberately set out to place their profession upon an objective and scientific footing. One of the costs of this commendable effort has been the studied avoidance of those issues which had made Irish history an arena for polemic and filiopietism. In the entire file of *IHS*, for example, one can find only a handful of articles which deal more than peripherally with the Catholic Church since the seventeenth century. Only in the past decade have professional historians begun to write the modern history of the Irish Catholic Church, and O'Farrell has made the first attempt to use the Church's history as the backbone for a general history of Ireland. It is an important step which deserves to be taken quite seriously even though the author's singleminded devotion to his theme has led him into some idiosyncratic arguments and questionable interpretations.

An example is his treatment of the Church's remarkable accommodation with revolutionary nationalism after 1916. He rightly notes that the "constitutional" Irish Parliamentary party had lost some clerical support through its associations with English Liberal secularists and that the party's revolutionary rivals struck some responsive chords among the clergy. Yet he oversimplifies by relying heavily on the opinions of three outspoken prelates: Cardinal Logue of Armagh, Archbishop Walsh of Dublin, and Bishop O'Dwyer of Limerick. Significant as these three churchmen were, they were only three out of some 28 bishops, and in the years before the war they were notably out of step with their colleagues. Logue had been the patron of a political anachronism, T. M. Healy, until 1910; O'Dwyer had been a notorious maverick within the hierarchy since the 1880s; and Walsh had withdrawn after 1905 from active politics. The remainder of the hierarchy, by and large, cooperated with the Irish party, following the lead of the energetic Bishop O'Donnell of Raphoe (Logue's eventual successor in the Cardinalate), whom O'Farrell virtually ignores. To be sure, Logue, Walsh, and O'Dwyer regained the initiative within the hierarchy after the 1916 rebellion, but their earlier political eclipse is equally significant. The Church learned not only how to live with revolution, but how to

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live with parliamentary democracy as well, and the Irish Parliamentary party was not the irrelevancy O'Farrell would have us believe. Perhaps this is the reason that although the narrative purports to extend to 1970, O'Farrell seems to run out of things to say after 1923. Still it is a pioneering work which poses many of the questions the next generation of Irish historians must address.

DAVID W. MILLER
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The British General Election of 1970 *by David Butler and Michael Pinto-Duschinsky. New York, St. Martin's Press, 1971. —xvi, 493 pp. $15.00.*

The eighth Nuffield study of a British general election invites comparison with its predecessors. As might be expected from the commitment to record the essentials of a recurrent historical event and from the continued leadership of David Butler, there are important similarities in coverage, method, and tabulations. Those of us who have been grateful for the convenience of the previous volumes will have the same good academic reasons to be grateful for this one. Moreover, the authors add new and innovative material.

By making their volume more than ninety pages longer than any previous general election study, Butler and Pinto-Duschinsky are able to add a great deal without sacrificing any of the most useful information characterizing the earlier studies. The principal deletion is of the several constituency campaign studies that had been reported as a kind of particularized complement to the otherwise largely national story. Instead, however, there are now synthesizing chapters on constituency preparations and campaigning. These chapters are based on broadly gathered data rather than on a few particular constituencies. Here, as elsewhere in the volume, interviews and direct observations are used along with printed sources. Thus there is a partly inside story of the election and of the political events of the four preceding years—the latter heavily emphasized in explaining the circumstances of the election.

The appendixes, always significant in the Nuffield volumes, are substantially lengthened this time. Included are certain social and economic characteristics of each constituency, now that the British census provides such data, and also an ecological analysis based on these characteristics in relation to the election results. Michael Steed's statistical analysis of the results remains, as in previous volumes, a model of succinctness

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and clarity, notably in pointing out the growing big-city concentration of Labour strength and the growing suburban and rural concentration of Conservative strength. Steed also deals fully with the widely held view that the abstention of Labour voters caused the unexpected Labour defeat; he concludes from a comparison of constituencies that difference in turnout cannot explain Labour's poorer showing in 1970 than in 1966. He does note, however, that the relative apathy of Labour voters might account for the party's poorer standing in the actual election of 1970 than in the polling surveys that preceded the election. These surveys, which had led almost everyone to assume a Labour victory, are themselves analyzed in a separate chapter, where the major implication seems to be that the preelection polls might well have been accurate enough as of the date of their interviews but that most of them did not pick up the evidently sharp movement of opinion toward the Conservatives during the last few days.

Much more ought to be said about the richness of this book as a source of knowledge about contemporary British politics. For example, the authors document the significant decline of membership in constituency party organizations, leaving many additional units in the skeletal or cadre category that we used to regard as uncharacteristic of British politics. And, to cite another kind of illustration, the study tells us of recent changes in British electoral law, including one that allows candidates to put their party affiliation beside their names on the official ballot.

LEON D. EPSTEIN
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The British Ombudsman *by Frank Stacey. London, Oxford University Press, 1971.—viii, 351 pp. $13.00.*

The ombudsman—an independent official who receives complaints against government officials and agencies, conducts an investigation, and (if the complaints are justified) makes recommendations to rectify the complaints—is in office in Sweden (three), Finland, Denmark, Norway (two: a civil and a military), New Zealand, Guyana, Tanzania, Great Britain, Mauritius, Northern Ireland (two: a central government and a local), Israel, West Germany (military), and Switzerland (military). The Fiji and Ghana constitutions have provisions for creation of the office, and legislation is now being considered in The Netherlands. In Canada, the provinces of New Brunswick, Quebec, Alberta,

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Manitoba, and Nova Scotia have ombudsmen. In the United States, legislative created and appointed ombudsmen are in office in Hawaii and Nebraska, and executive appointed obudsmen are in office in Iowa and Oregon. In New Mexico, the lieutenant governor has been given by statute general ombudsman functions.

The first application of the ombudsman system with tailor-made variations to a large populous country was the British institution known as the Parliamentary Commissioner for Administration. To the best of the reviewer's knowledge, this is the first full-length book to be devoted to the British ombudsman, so proclaimed even though the legislation does not use the term and the government initially struggled to protest that the commissioner was not an ombudsman. Author, Frank Stacey, is a member of the department of political theory and government in the University College of Swansea, Glam, South Wales, and his book is a valuable contribution to the study of the ombudsman.

Stacey traces the origin of the British concept, through the vital role of justice (the British section of the International Commission of Jurists), the seminal Whyatt Report, the position of the Conservative party, which was not attracted to the justice proposal, to the favorable position of the Labor party, which on coming to power in 1965 decided to introduce the necessary legislation, and finally the drafting and passage of the Parliamentary Commissioner for Administration Act in 1967. The careful catalog of these details is reflected in the fact that 235 pages were utilized.

The operation of the office in four years is covered in rather summary fashion in ten pages, with eleven additional pages being devoted to the Sachsenhausen case which may be said to have changed the commissioner from an "ombudsmouse" to an "ombudsman."

It should be noted that the first commissioner, Sir Edmund Compton, retired on March 31, 1971, and his successor, Sir Alan Marre, took office on April 1, 1971. Therefore, the evaluation of the Parliamentary Commissioner for Administration (covered in 36 pages) must be limited to the tenure of Sir Edmund Compton. An English review of this book stated that the British institution "has not in fact put on much weight during its first four years." This is somewhat unkind because regardless of the personality of the incumbent, there were weaknesses and concessions built into the office under the legislation which created it. Nevertheless, the Select Committee on the Parliamentary Commissioner for Administration and the Parliamentary Commissioner for Administration should give consideration to the evaluation by Mr. Stacey, particularly with respect to the criticism (made by others as well) that the Parliamentary Commissioner for Administration should be better able to

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publicize his findings.

The Select Committee on the Parliamentary Commissioner for Administration, which plays a strong role in evaluating the work of the Parliamentary Commissioner for Administration and in making recommendations, is covered in 48 pages.

That Great Britain thinks well of its system is reflected in its extension to Northern Ireland which has both a Commissioner for Complaints, handling complaints against hospitals and local government, and a Parliamentary Commissioner for Administration, handling complaints against central government; in the announcements by the government that it proposed to introduce legislation to establish an ombudsman system for local government in Great Britain and that it had decided to create a Health Service Commissioner for England, Scotland, and Wales.

It would have been helpful to the reader to have available the Parliamentary Commissioner for Administration Act, 1967, as an appendix. On balance, *The British Ombudsman* is an effective tool for those seeking information as to the origin and creation of the Parliamentary Commissioner for Administration in Great Britain.

BERNARD FRANK
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Spear and Scepter: Army, Police and Politics in Tropical Africa *by Ernest W. Lefever. Washington, D. C., The Brookings Institution, 1970.—x, 251 pp. $6.50.*

African Armies and Civil Order *by J. M. Lee. New York, Frederick A. Praeger for the Institute for Strategic Studies, 1969.—ix, 198 pp. $6.00.*

The phenomenon of military coups in Africa continues to fascinate scholars. These two books are important but not outstanding contributions to the subject. They both in their different ways seek to delineate the present level of our understanding as well as to stimulate new lines of thinking.

Of the two, Lefever's book is at once less solid and more provocative. Lefever is fairly daring in some of the hypotheses he puts forward. A number of them are familiar to students of coups and of Africa, but on occasions, Lefever provocatively challenges received "truth." For example, agreeing that civic projects conducted by the armed forces—such as literacy programs and roadbuilding—can contribute to development in

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an African country, Lefever nevertheless provides a brief cost-benefit analysis of this approach to warn against excessive optimism: "On the economic side, a soldier probably costs from three to five times as much as a civilian to do the same job because of the considerable military overhead. Compare, for example, the per capita cost of a civilian laborer or engineer with that of his military counterpart." Lefever concedes that if the soldiers are otherwise idle in the barracks, using them for development projects would make for greater economic rationality, but, he says "the best way to deal with redundant military manpower is to cut down the size of the army and utilize the resources saved to employ qualified civilians."

Lefever's logic is sound here, but what he recommends is much easier said than done. The escalation of military expenditure tends to include also the sheer expansion of the armed forces. Civilian governments would very rarely dare reduce either the privileges or size of their armed forces. Military regimes have so far proved equally incapable of pruning their forces. Nigeria has yet to find a way of reducing the vastly expanded army whose functions are now fewer since the end of the civil war.

Lee is more comprehensive in his discussion of what he calls "the link between labor policy and security policy" (p. 132). Lee maintains that *discipline* is critical both for development and for security: "How far could discipline be given a new significance after independence? Could it provide a new means of bringing the security forces and the community closer together?" He discusses the quest for new forms of national service. Newly independent states could either direct the energies of the security forces toward a program of nonmilitary public works or extend the scope of military training to include new ideas of national service. Lee sees possibilities of hybrid solutions, but he too is aware of the costs of this endeavor: "Few states could afford to create a national service which required each citizen to enroll in a disciplined organization, not only because of the heavy costs involved in making it nation-wide; but also because of the risks which such an organization might bring to the delicate balance of the new political system" (p. 141). In fact, Milton Obote on the eve of his overthrow from power had intended to create precisely such a national service for Uganda. It is a matter of interesting speculation whether such a service would have turned out to be too costly to be manageable. It is also a matter of speculation whether an important factor precipitating the Uganda coup was the concern of the armed forces over the implications of Obote's proposal. Certainly the Ministry of National Service was one of the first institutions liquidated by the new republic in Uganda.

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J. M. Lee's concept of political development generally is more sophisticated than Lefever's. Lefever is still very much wedded to a law-and-order definition of political development: "Political development is defined in this study as any change in the structure of government or the process of gaining and exercizing political authority that enhances the government's capacity to provide security for its people and to enforce the law throughout its territory" (p. 174).

Lee correctly designates such a concept of development as basically "colonial" in African conditions and would use the expanding of consensus rather than the consolidation of security as the proper measure of political development (see especially p. 156). On this Lee is the more persuasive of the two authors.

Lefever's ideas on the problems of consensus and solidarity within the armed forces themselves are illuminating, but he seems to have swallowed a little too uncritically the thesis that "the army as the most detribalized and most modernized institution in the state is a living, if imperfect, example of one nation." Lefever seems to take it for granted that military life and its opportunities for ethnic mixture would provide a model for interethnic fusion.

Ethnic intermingling does take place in the barracks, but it is hazardous to conclude that it results in reducing tensions between ethnic groups. Lefever underestimates the tensions generated by this intermingling, which precipitated the violent eruptions in Nigeria in 1966 and the brutal events in Uganda in 1971, when soldiers fought soldiers. But Lefever's three case studies are on the whole well handled. The choice of countries was quite fortunate—Ghana, an English-speaking country with considerable military professionalism; the Congo, a French-speaking country which started with a rabble army; and Ethiopia, in some ways more like a Latin American country than an African country, at least in the relationship of forces between landowners, the church, and the military. Lefever attempts to see military behavior and performance in the wider context of history and political geography. He provides useful appendixes of selected facts concerning the three countries and information about American military assistance agreements. There are some errors and superficialities. It is not true that the army in an African country ruled by a military regime is a more popular institution than the police; it is not true that the Somalis are not divided by tribes and tribalistic tensions; and, at a more petty level, we might note that the Cambridge-based journal on African affairs is called *Journal of Modern African Studies* and not "Journal of Modern African Affairs."

But these are minor points. Both Lefever and Lee deserve our thanks

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for defining further areas of investigation and reflection, and students of civil-military relations in Africa would do well to consult these books before going any further in their explorations.

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Uganda

The Nyoro State *by John Beattie. Oxford, Clarendon Press, 1971. —ix, 280 pp.*

Bunyoro at its prime was an important kingdom by any standards, holding some sort of sway over much of what is today Uganda. However, it suffered badly from the intrusion of the Europeans. For a variety of reasons its fortunes declined in the early phases of the colonial period, while its neighbor Buganda exploited the new situation so successfully that it established what has been described as a "sub-imperial" dominion within colonial Uganda.

When Beattie studied them in the early fifties, the Nyoro still suffered from low morale, but a new class of landed gentry had emerged, together with the inevitable "new men" of all colonial countries; and in 1955, they negotiated a revised agreement with the British which entrenched their position at the expense of that of the king, not to mention the peasants. The relationship between this event and the contemporary misfortunes of the Kabaka of Buganda is not explored here, but the later history of the kingdom is necessarily (if briefly) related to the decisive events in Buganda, where the ruling class took an extraordinary gamble only to lose everything to Obote, bringing down the other "feudal kingdoms," including Bunyoro, with them. Since then there has been another resort to force in Uganda. Obote has followed the Kabaka into exile, and, it seems, the kingdoms are recovering some lost ground.

Beattie is the leading foreign authority on the Nyoro, and this long-awaited book will be generally accepted as the standard work on the Nyoro state. It is scholarly and well written, and it covers a good deal of ground. These are great virtues, and it is perhaps ungracious to carp when an author achieves so much. However, in several important ways this book is a disappointment. Had it appeared within five or ten years of the fieldwork, it would have been regarded as a major work; but now, nearly twenty years after, it is dated, not only in its matter, but much more important, in manner.

There are certain minimum demands one makes of a modern study

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in political anthropology. It must not only be historical rather than timeless, but it must also ask the important questions: *Cui bono?* And how does the system work for those who are working it? To put it another way, it must have something to say to the political scientist.

Although Beattie is concerned with process in several of his chapters, the study lacks a sense of history. History is absent, for example, in the chapter on the traditional state, and the reader is never quite sure when tradition happened. Beattie includes an interesting account of the Mukama's loss of certain powers, but he does not really explain why the educated elite felt that this was in their interest. Indeed, the interests of "the Nyoro" are too readily presented as though, by and large, they were uniform, the peasants, mesmerized by ritual and unquestioningly deferential, simply accepting the exactions of the rich and powerful. But what were the sanctions? Finally, the description of the system of power is too formal by modern standards. One is left without a sense of how decisions were actually made at any level in the system. In short, Beattie never provides a satisfactory answer to Laski's question, Who gets what, when, and how?

The book is based on articles written over many years. This explains in part the absence of a modern theoretical perspective and also the lack of integration of the parts. Further, the fieldwork was carried out mainly among peasants, and events at the centers of power were not often observed directly. One hopes that the promised monographs on community and ritual will be more contemporary in inspiration, as they will certainly rely more upon Beattie's own observations. If so they will not only be welcomed by students of Uganda and of the Interlacustrine kingdoms, as this book deserves to be, they will also help develop modern anthropology, as this book should have done.

ADAM KUPER
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Egypt Under Nasir: A Study in Political Dynamics *by R. Hrair Dekmejian. Albany, State University of New York Press, 1971. —xvi, 368 pp. $10.00.*

Rural Politics in Nasser's Egypt. A Quest for Legitimacy *by James B. Mayfield. Austin, University of Texas Press, 1971.—xvi, 288 pp. $8.50.*

We are lucky to have these two very good studies of Nasser's politics so

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soon after his death in September 1970. Both seek to describe and explain Nasser's relation to the Egyptian people and the political mechanisms by which he (as the active element in the mixture) expressed it. The two studies, begun before Nasser's death, try to sum up his effect and legacy, but the authors have somewhat different emphases. Dekmejian deals with internal politics on a national scale, international politics on a regional and world scale, and with the nature of the leaders Nasser brought to very limited power to help him. Mayfield deals with only internal politics, specifically in the villages. The strong point in both books is their descriptions of events and their summaries and analyses of policies, contributions only slightly vitiated by a certain penchant for old and new concepts in the social sciences that are introduced often but without much useful effect.

Dekmejian's main concept is that of charisma, with a discussion of which he introduces the book. He concludes that during 1952-70 Nasser "achieved a degree of internal legitimacy and stability uncommon in the world of developing nations" (pp. 226-27). Internally, at least, he succeeded in meeting his greatest test following the war of June 1967, but he was less successful in the "shift in the revolutionary dynamic from internal affairs to foreign politics. . . ." Dekmejian feels that Nasser lost by delaying the application of a new ideology (beyond nationalism) to internal affairs and the creation of "organizational structures" for "conflict resolution." Behind the social science jargon, the author seems to say that socialism as an ideology and the creation of a vanguard party and socialist institutions were not pursued early and vigorously enough. This attitude seems to me to exaggerate the possibilities of ideology to cultivate the kind of values, such as freedom and self-rule, which the author apparently favors.

Mayfield is more realistic on this score, I think, perhaps because he studied Nasser's rural politics by spending time in villages and seeing first hand the problems of a regime that wants to move masses. Mayfield likes fashionable concepts, but escapes their worst effects by reporting what he has actually seen in the rural areas where the national policies were applied. He thus analyzes the "traditional village leadership system . . . , the single political party system (the Arab Socialist Union), and the local administrative system (village councils)" (p. 10). Though a foreigner, of course, he is not awed by the usual claim that such observers cannot penetrate to the village, for he points out that the Egyptian elite groups are likewise "strangers to the values, attitudes, and motivations" of the peasants (p. 5). (In Dekmejian's book, several useful analyses of the makeup of these elites support this point.)

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After a sympathetic and thorough review of elite (if revolutionary-sounding) politics in the villages, Mayfield concludes that they failed because the masses did not really want them nor did the regime have the conviction or power to impose them (p. 173). Despite the official effort to cover up shortcomings, the author says his pessimistic conclusion is "substantiated by several Egyptian sources who have objectively analyzed the rural programs . . ." (p. 184). He shrewdly notes the difference between a regime's intentions and its achievements (pp. 171, 178-80), and offers the judgment that "Nasser's charisma rested more on his international exploits" than his internal plans and activities (p. 97). The gap between programs and achievements is nowhere clearer than in Mayfield's account of the Arab Socialist Union in the villages he studied (chaps. 6, 7). This sort of gap may be found, of course, in all kinds of countries, whether or not they are developed, or democratic. The issue is, of course, the degree of public participation in setting the goals, controlling the means, and evaluating the results—and here Nasser's Egypt fell short.

Both books are based on close knowledge of Egypt and on many primary sources, and the authors combine objectivity with sympathy for what Egyptians have been attempting. To this reader, at least, Mayfield's greater sense of realism comes from his having had the advantage of seeing personally what he discusses.

MORROE BERGER
Princeton University

Rebellion and Its Enemies in Late Imperial China: Militarization and Social Structure, 1796-1864 *by Philip A. Kuhn. Cambridge, Mass., Harvard University Press, 1970.—254 pp. $8.50.*

Social history has become one of the frontiers for the study of China's past, and rural society is the focus of attention. In 1960, Professor Hsiao Kung-chuan published a masterly work, *Rural China: Imperial Control in the Nineteenth Century*, which laid open the wide field of China's rural social and economic structures, pioneering on the ever-expanding frontiers, the prevalence of organized violence, and more particularly the relationship of the imperial system to the vast population in villages and towns strewn throughout a continental country. Other scholars have also done seminal work probing into various aspects of rural society—Ho Ping-ti, Chang Chung-li, Franz Michael, Maurice Freedman, G. William Skinner, and Frederic Wakeman Jr.—to mention

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some of the more exciting among the historians and social scientists. The latest exploration, and a fine one, is Philip A. Kuhn's study of the manner in which rural elites organized military power locally to protect their interests and to cope with the mid-century challenge to the established social order.

After recalling China's various earlier systems of military organization for local and frontier defense and describing the progressive breakdown of rural order in the half-century between the White Lotus Rebellion and the Taiping eruption, Professor Kuhn provides a systematic account of the *t'uan lien* or local militia system. This he generalizes on the basis of several case studies drawn from hitherto little noticed primary sources. He has also made impressive use of secondary works by modern Chinese and Japanese scholars.

The focus of his analysis is the instrumentalities by which rural leaders, themselves linked through a hierarchical structure, knit together their village corps into wider regional militia sometimes numbering thousands of braves. The various levels of militia organization he names "simplex," "multiplex," and "extended multiplex" corps, terms which seem likely to enter the historians' vocabulary, since they are appropriate for other forms of rural organization. He also shows how heterodox elements within rural China—secret society lodges, bandit gangs, and rebellious religious sects—developed similar hierarchies.

In the local and regional contests which went on constantly, the rural elites, "scholar-gentry" and quasi-gentry, usually had the advantage because they interlocked through the traditional academic system with the lower levels of the bureaucracy and with the state itself. With their well established lateral and vertical communications systems, they were able to create an integrated military structure from local guards up to massive regional armies far more effectively than could gangsters, outlaws, and rebels. The test came with the Taiping war, the military aspect of which Professor Kuhn examines with a fresh eye.

On a broader level, the study helps explain the inertia-like stability of the Chinese state during the nineteenth century despite great challenges from without and fatal weakness at the apex of the structure. And what about the republican period? What happened to the rural elites with the ending of the examination system which shaped their outlook and of the imperial system which provided their ultimate sanction? This is a challenging question which Professor Kuhn puts on the agenda; so far there are only ill-based generalizations and a few empirical local studies to go upon. Yet certainly the role of local elites was part of the reality behind the facade, as it were, of urban-based and "modern" republican China. Even the problem of how the most recent rebels, the

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Chinese Communists before they became the orthodox rulers, rose to power in the hinterland should be illuminated by Professor Kuhn's valuable study.

C. MARTIN WILBUR
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India's China War *by Neville Maxwell. New York, Pantheon Books, 1970.—475 pp. $10.00.*

"India's China War" was, specifically, the major military clash of 1962 on the Indian-Tibetan border. Neville Maxwell was eminently qualified to treat the subject. He was the London *Times* South Asia correspondent, stationed at New Delhi from 1959 to 1967, and thus viewed the Sino-Indian border conflict at close range. He later buttressed his store of facts by wide research—especially, in Indian sources. His coverage of the Chinese side is much less extensive. But access to Chinese documentary materials would presumably have made no essential difference in his overall conclusions: on the basis of the *Indian* evidence, he condemns Indian policy.

In a historical introduction, Maxwell defines the background situation: India's borders with Tibet as inherited in 1947 from the British empire were legally unsettled and disputable with respect to both the McMahon Line and Ladakh. When China soon after reasserted its control over Tibet, the potential for dispute became an imminent political reality.

Maxwell traces the developing quarrel in great detail. There is sometimes more detail than needed to establish the point and more repetition than warranted even by a literary structure requiring (as in Part III) some retreading of familiar ground. But the main theme is clear and cogently supported. The Indian political leadership, Maxwell demonstrates, was committed by nationalistic sentiment to maintaining without compromise, or even meaningful negotiations, claim to the farthest reaches of British imperial dominion on the Tibetan borderlands; whereas Peking expressed its readiness to negotiate a compromise which, at least at an early stage, envisaged China's abandoning its claim to the Northeast Frontier Agency (NEFA) area south of the McMahon Line in exchange for a clear title to the Aksai Chin plateau on the Ladakh border. When, in the face of repeated Chinese warnings, India began to press forward *militarily* to make good its claims, the die was cast. The end result was the one-month "war" of October 20-November 20, 1962, which brought disastrous defeat to the Indian arms—and the restoration of the status quo ante (in Chinese terms) on both east and west, *without* political

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agreement.

Maxwell's account graphically depicts the extent of Indian miscalculations, in both the political and military realms, of Chinese intentions and capabilities. It is clear today that New Delhi would have been better advised to give up the Aksai Chin plateau (important for China, practically worthless for India) in exchange for formal confirmation of Indian sovereignty over the NEFA.

The story, so thorough for Indian aims and actions, necessarily remains incomplete because of the limited treatment of the Chinese side. In a deductive analysis (pp. 346-50) of Chinese motivations, Maxwell touches upon analytical possibilities which he does not fully exploit in his work. If his strictures against New Delhi's policy and strategy appear well merited, in this reviewer's estimate he credits Peking with too much political virtue, and too little geopolitical thinking, in the affair. This said, be it further remarked that Neville Maxwell has perceptively and effectively related the history of India's "China War" as it was to be seen from the Indian side. That history constitutes an enlightening lesson in Asian politics—in many respects.

O. EDMUND CLUBB
Palenville, New York

North Vietnam's Strategy for Survival *by Jon M. Van Dyke. Palo Alto, Pacific Books, Publishers, 1972.—336 pp. $10.95.*

This is a timely book, since the United States has again embarked on a policy of trying to bomb the Democratic Republic of Vietnam into acceptance of American peace terms. Dr. Van Dyke's study deals with the utterly futile attempt from 1965 to 1968 to bomb these Vietnamese to their knees. It is an account of just how the victims coped with the death and destruction dropped on them—the heroic and strenuous measures they took to minimize the loss of life, care for the mutilated, and keep the wheels of industry going by moving plants into caves if necessary and substituting human power for electricity. The book describes the steps taken to ensure the transportation of necessary supplies for the war in the South against the U.S. Army and the Saigon regime which cooperates with it. This movement of goods south required the organization of large numbers of people to carry supplies—on their backs, bicycles, and on trucks—and to repair roads and bridges. Much of the story is already familiar to newspaper readers. Yet even so, such a vast panorama of human suffering, hardships, and deprivation, prodigious effort, and superhuman endurance is revealed in this well researched book that its

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impact is overwhelming.

The Vietnamese come through as a truly extraordinary people able to persevere despite the endless need for grueling and dangerous labor by young and old, men and women. These people are depicted as courageous, disciplined, resourceful, enterprising, hard-working, intelligent, with a gift at improvization, and an ability to learn from experience. And they are, above all, heroic.

While Van Dyke's book documents this heroism, it throws little light on the question: why are they so heroic, what gives them their strength? He says that their leaders enjoin the people to be patriotic. So do the generals in Saigon—but to no avail. The book echoes the usual clichés of official American analysts, such as Douglas Pike, that Communist leaders are very clever in manipulating people and that theirs is a triumph of organization. This, of course, ignores the fact that the Saigon military have tried with might and main to manipulate the Vietnamese people and that all the techniques of organization which the Communists have used to enlist people in a common cause are readily at hand for the Saigon military, if they but choose to use them. The contrast between Saigon and Hanoi seems to suggest that the Democratic Republic of Vietnam has a secret weapon, namely, socialism and the enthusiam it inspires—not secret at all to the Vietnamese but apparently beyond the grasp of most Americans.

Another wellspring of Vietnamese heroism is the nation's history which is hardly touched on in this book. To the Vietnamese in the North and their colleagues in the National Liberation Front in the South, the American solution of two separate Vietnams, a Communist state above the Seventeenth Parallel and below it an anti-Communist state within the U.S. orbit, is no solution at all. It violates national sentiment and the people's long struggle to be a united country free of all foreign domination. Vietnamese patriots feel that the American presence in the South is not only a violation of the South's freedom but a threat to the North as well. There is the ominous French precedent. History-conscious Vietnamese remember how in the nineteenth century, France used her dominion over the South as a springboard for conquering the rest of Vietnam. Surely, an important key to the extraordinary endurance of the Democratic Republic of Vietnam has been its belief that the cause of freedom is a just cause that will ultimately prevail.

HELEN B. LAMB
New York City

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Laos: War and Revolution *edited by Nina S. Adams and Alfred W. McCoy. New York, Harper & Row, Publishers, for the Committee of Concerned Asian Scholars, 1971.—xxiii, 482 pp. $15.00.*

This is a stimulating, uneven, and altogether provocative collection of essays on the tragedy of Laos.

An attempt to review the collection in the allotted five hundred words risks notable distortion, not least because this is committed scholarship and it is impossible to imagine a review written without a political commitment of some sort on the part of the reviewer. For an opponent of United States involvement in Indochina, such as the present reviewer, this collection of essays provides confirmation and documentation of much that was already known, suspected, and even unsuspected about the history of United States attempts to impose American solutions within the Laotian vale of sorrows over the past fifteen years.

For those who doubt that such an involvement took place, the collection will be a bucket of cold water thrown to wash away the dust of preconceptions, misconceptions, and sheer ignorance of the manner in which the world's greatest superpower has so ineptly—to use the most charitable interpretation—pursued its curious goals at the staggering cost of death and destruction in one distant region of the world. And for those who believe that United States involvement in Laos was, and is, right, it is, one must suppose, most unlikely that their frame of reference will permit them to accept the all too valid picture which this volume presents of error, compounded by deliberate disregard of fact, and of arrogant assumptions, pursued in the face of all available evidence, in the U.S. attempt to turn Laos into what it can never be—a viable state, ready to fight for the supposed ideals of the "free world."

Possibly the most important contribution of the volume is the manner in which it redresses the balance of scholarship and writing, for the two are clearly not always the same, on Laos. Vietnam and its tragedy have inspired a vast body of publication over the past decade. Attention to Cambodia has been considerably less, but it has been substantial nonetheless. Laos has seldom emerged into the lack of obscurity, to misquote Senator J. William Fulbright, which it deserves. While only rash and thoughtless students of Laotian affairs would disregard the writings of the late Bernard Fall, of Arthur Dommen, Joel Halpern, and Hugh Toye, there is strong reason to argue that the volume under review offers a more significant series of insights into the nature of conflict and rivalry in Laos.

Consider opium. Most interested observers have been aware that the

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opium trade has been, and is, a significant factor in Laotian politics. But David Feingold's essay, "Opium and Politics in Laos," is by far the best summary of the issue to date. Other contributions, in particular those by D. Gareth Porter and Fred Banfman, dealing with the recent history of American involvement in Laos, highlight the full extent of double dealing and straightforward deception on the part of successive American administrations. Not that these commentators are themselves free from fairly obvious prejudices. One cannot help feeling that for these, and other contributors, the aims and actions of the Democratic Republic of Vietnam are consistently viewed in the best possible light.

While recognizing the value and utility of much of the background information provided in such essays as those by John K. Whitmore, Alfred McCoy, and Nina S. Adams, this reviewer found one contributor's essay, above all, outstanding in the way it provided an epitome of America's tragic involvement in Laos, John Lewallan's essay on the International Voluntary Services's work in Laos. It demonstrates better than anything else how American ideals could distort understanding of the political factors involved in the Laotian situation and how adherence to vaguely defined concepts of "freedom" could pervert the actions of men with a genuinely disinterested commitment to the improvement of the lives of others.

Absurdly expensive though this book is in its hardcover edition, it is an essential piece of reading on the sadly neglected subject of the even sadder destruction of a state.

MILTON OSBORNE
Monash University
Australia

The Alliance That Lost Its Way: A Critical Report on the Alliance for Progress *by Jerome Levinson and Juan de Onís. Chicago, Quadrangle Books, Inc., A Twentieth Century Fund Study, 1970.—xi, 381 pp. $7.95.*

This study is a critical appraisal of the Alliance for Progress by a former senior U.S. administrator of the program and a distinguished *New York Times* reporter with many years of experience in Latin America. It provides background on Latin American affairs before the advent of the Alliance. It discusses many aspects of the Alliance in action, such as the role of U.S. business, the limitations of foreign aid, and the efforts to accelerate regional economic integration. It reviews successes and failures in the fields of social development, land reform, urban

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development, and education. Throughout, the study relies heavily on the personal experience of the authors and on recent interviews with former U.S. participants in the Alliance and, to a much more limited extent, on interviews with Latin Americans. In an epilogue, the authors make recommendations for future U.S. policies toward Latin America.

The overall conclusions are forceful but not highly controversial: "The Alliance has yielded more shattered hopes than solid accomplishment, more discord than harmony, more disillusionment than satisfaction" (p. 307); "between the overambitious idealism of its development goals and the pointless obsessiveness of its concern for security, the United States really undermined the Alliance before it could get started" (p. 323). In support of these conclusions, the study provides rich, intimate detail on the behavior of many U.S. officials, such as Assistant Secretaries of State Thomas Mann and Lincoln Gordon, and on the U.S. and Latin American political environment surrounding many specific projects, programs, and decisions. With a journalistic touch, the authors illustrate their views with selected incidents which rely heavily on their special expertise about the Brazilian situation.

The Alliance was hastily conceived and overly ambitious, the authors conclude. It had conflicting goals, and the U.S. opted frequently for superficial and short-run gains. Furthermore, the reach of the U.S. exceeded its grasp. Many of the Alliance aspirations were beyond the power of external foreign assistance to accomplish.

The major contribution of the study is its analysis of the role of the U.S. It makes virtually no use of the extensive writings by Latin American scholars and government officials evaluating the Alliance, at least as judged by the footnotes. The study is weak in economic expertise when appraising agrarian reform, the distribution of the benefits of economic growth, migration, and urban development, the costs and benefits of foreign investment, and many other complex subjects. In particular, the study does not include a summary of the remarkable economic progress that Latin America achieved during the 1960s—Alliance or not.

Given the backgrounds of the authors, it is not surprising that a "Latin American Crisis" syndrome pervades the study. Probably the "crisis" flavor results because political patterns in Latin America have not been following narrow conventional models. Political ferment is uncomfortable for U.S. businessmen and government officials, but by Latin American standards, such ferment does not necessarily constitute a crisis. Latin America has become highly experimental in political approaches that hopefully will lead to improved political machinery for serving the unique needs of Latin America.

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Levinson and de Onís have made available to policy makers and scholars a rich collection of material and insights on one of the most significant experiences in U.S.-Latin American relations. The U.S. can benefit tremendously by better understanding its past failures. But will it?

STEFAN H. ROBOCK
Columbia University

Transnational Relations and World Politics *edited by Robert O. Keohane and Joseph S. Nye Jr. Cambridge, Mass., Harvard University Press, 1972.—428 pp. $15.00.*

The theme of this interesting and important symposium is—as the editors state in the introduction—to overcome the excessive concentration of political scientists on the relationships between states and a "state-centric view of world affairs." It is the purpose of the present volume to correct this one-sided preoccupation, by analyzing the many transnational actors and forces that operate outside the realm of the state or in complex interaction with it. The editors have grouped these questions into five broad areas of inquiry:

> 1) What seems to be the net effect of transnational relations on the abilities of governments to deal with their environments? To what extent and how have governments suffered from a "loss of control" as a result of transnational relations? 2) What are the implications of transnational relations for the study of world politics? Is the state-centric view, which focuses on the interstate system, an adequate analytic framework for the investigation of contemporary reality? 3) What are the effects of transnational relations on the allocation of value and specifically on asymmetries or inequalities between states? Who benefits from transnational relations, who loses, who controls transnational networks, and how is this accomplished? 4) What are the implications of relations for United States foreign policy? Insofar as the United States is indeed preponderant in transnational activity, what dangers as well as opportunities does this present to American policymakers? 5) What challenges do transnational relations raise for international organizations as conventionally defined? To what extent may new international organizations be needed, and to what extent may older organizations have to change in order to adapt creatively to transnational phenomena? (p. xi)

The book is divided into four major parts: I. Transnational Relations: The Nature of the Beast; II. Transnational Organizations; III. Issue Areas; IV. Towards Peace and Justice? The authors are an impressive group of political scientists and economists, although the total omission of international lawyers, who, in the postwar era, have contributed a great deal to the conceptual study of transnational law and transnational

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organization, is a rather glaring deficiency.

In a short review, it is impossible to do justice to the many fields or to the multiplicity of approaches. Although such noneconomic transnational institutions as the Roman Catholic Church and the Ford Foundation are discussed in the essays, the predominant preoccupation is, not so surprisingly, with "transnational economic processes" (the title of an essay contributed by Edward L. Morse) from various angles. The essays contributed by Robert Gilpin ("The Politics of Transnational Economic Relations"), Louis T. Wells Jr. ("The Multinational Business Enterprise"), Lawrence Krause ("Private International Finance"), Peter B. Evans ("National Autonomy and Economic Development: Critical Perspectives on Multinational Corporations in Poor Countries") are variations on this theme. One of the most searching and interesting of the contributions is by Robert W. Cox, formerly director of the International Institute of Labor Studies of the ILO and the newly appointed Professor of International Organization at Columbia University, on "Labor and Transnational Relations." This essay illustrates—as do many of the others—the complexity of the so often oversimplified question whether nationalism or internationalism prevails in the trend of contemporary world politics. Of course, the pre-1914 ideology of a labor movement based on the international solidarity of the working classes—a transnational image of Marxist vintage—symbolizing the opposition of the exploited working classes to the bourgeois national state, exploded with the outbreak of World War I and has never been revived. But present trends are contradictory. Organized labor in the developed countries tends to look with suspicion upon the trend of multinational corporations to transfer production to less developed countries, where labor is poorly organized, often autocratically controlled by governments, and underpaid. On the other hand, the international trade union movement has a strong interest in extending its organization and assistance to the poorer countries so as to improve the organization of labor in these countries and raise the level of wages—which, in turn, will diminish the gap between the labor costs in the developed and the undeveloped countries. To some extent, the very success of transferring production to less developed countries brings such an adjustment. In Japan, which of course is now one of the most highly developed countries, rising labor costs and the belated revaluation of the yen have to some extent diminished the gap, and in Singapore, where labor is strictly controlled by the government and which is at a less advanced stage of development, the gap is also gradually diminishing. But, generally, national links are still far stronger than transnational ones and, as Robert Gilpin argues, "in direct confrontation with transnational actors, governments generally prevail" (p. 372). As Wells, Vernon, and

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others point out, the popular picture of the multinational corporation as a giant octopus, whose tentacles reach out to the remote corners of the world from a central power center—usually located in the United States—is greatly exaggerated. The counterpressure of host governments, which can singly or, more effectively, in combination control the conditions under which the locally incorporated subsidiaries operate, can often enforce joint ventures with local—public or private—majority participation, is far stronger than generally credited. On the other hand, organized labor in developed countries such as the United States, Britain, or France will tend to rely on its own government for protective tariffs, import quotas, or other retaliatory measures to inhibit the continuing transfer of productive resources to other countries.

Generally, the contributors tend to regard the multinational enterprise as a beneficial phenomenon, provided it is brought under some public control. Unfortunately, in recent years, governments have generally tended to weaken rather than strengthen international cooperation of the kind that would blend the mobility of capital and labor with the protection of the interests of the developing countries. Developments in the law of the ocean bed are a telling and deeply discouraging illustration of the trend toward competing nationalism rather than the community of interests in a world that depends on cooperation for its survival.

For any student of contemporary international politics and economics, this volume will provide rich inspiration and many valuable insights. That it leaves more problems unsolved than solved is not the fault of the editors or contributors but of the tragic gap between established modes of thinking—for which national sovereignty is still far and away the most important single focus of power and loyalties—and an increasingly interdependent world, which must learn to think transnationally, but probably will not do so before it is too late.

WOLFGANG FRIEDMANN
Columbia University School of Law

The First Fifty Years: The Secretary-General in World Politics 1920-1970 *by Arthur W. Rovine. Leyden, A. W. Sijthoff, 1970.—498 pp. Dfl. 59.*

The First Fifty Years is an important and yet in some ways a disappointing study. Arthur W. Rovine has compiled an impressive body of configurative information on the six secretaries-general of the League and UN in the first section of the book. In the second, Rovine attempts a

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start toward a functional theory of the office based on an analysis of tasks and resources.

In the historical section, there is much on which to comment. Perhaps most important is the author's interpretation of Sir Eric Drummond as exceedingly active and more influential than previously recognized. This line of thought is consistent with other recent studies, such as that of League Under-secretary Pablo de Azcarate in the *Tribune de Genève* of June 20, 1969. As Azcarate wrote to this reviewer on August 4, 1969, "I am sure that Drummond was not only as much, but even more active than Lie, Hammarskjold and Thant in the political field. . . ." Rovine's similar view of Drummond (see pp. 422-23), while stimulating, is not fully persuasive. If Drummond's "greatest contribution to world institutions and politics" was the "legacy of internationalism in the Secretariat" (p. 43), then the following factors detract from that accomplishment: the nonacceptance of internationalism by the major governments in the League period; Drummond's return to the British Foreign Office after 1933; Secretary-general Avenol's taking instructions from the Vichy government; Soviet attitudes in the UN era; *inter alia*. Rovine notes all of these factors, but his basic conclusions seem to underemphasize their impact on Drummond's record. More generally, there may be a tendency in this work to equate the secretary-general's participation in international diplomacy with influence. While Rovine makes clear Drummond's participation in most international crises, questions can be raised about Rovine's statement that "even the quiet Drummond had a great political influence" (p. 423). There remains much of merit in the historical section, and the analysis of the Thant period is both original and incisive, despite the problems of contemporary analysis.

The second section, an effort to construct "propositions to assist in understanding the nature of the Office . . . at what might be called a middle level of abstraction" (p. 415), leaves much to be desired. The work does not fully utilize other studies of the secretaries-general, although most are listed in the bibliography. For example, Oran Young's treatment of resources in *The Intermediaries* is both more concise and more systematic than Rovine's, yet it is generally neglected. Also, Rovine does not build upon, or explicitly reject or revise, other literature dealing with functional tasks. Rovine, Young, and Leon Gordenker (*The UN Secretary-General and the Maintenance of Peace*) all delineate the equivalent of three functional tasks. Two of the three seem the same in all the books —what Rovine calls the tasks of diplomacy and conflict management. But the differences among the three are obvious with regard to the third task. Moreover, Rovine does not give specific attention to studies by Mark

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Zacher and Robert W. Cox. Thus Rovine's work is not as cumulative and synthesizing as might be hoped.

Also, his tendency to borrow terms and concepts from systems theory and structural-functional theory may obscure as much as it elucidates. The secretary-general seems to be a complete political system in himself: "the Secretary-General's diplomatic tasks of communication, interest articulation, aggregation, and legitimization, all play a part in what might be characterized as his rule-making function" (p. 447). And so many tasks, roles, and resources are identified that the discussion becomes rather confusing (p. 449).

The First Fifty Years is likely to become a basic work in the field, in part because of the wealth of historical material systematically presented and in part because of largely accurate descriptive analysis. But Rovine's propositions about the secretary-general's functions, tasks, roles, resources, and influence are not likely to be generally accepted. They do provide another point of departure for those seeking more than descriptive analysis.

DAVID P. FORSYTHE
Georgia State University

White Racism: A Psychohistory *by Joel Kovel. New York, Pantheon Books, 1970.—vii, 300 pp. $7.95.*

One—even one who has experienced the effects of racism—ends this book with a sense of being overwhelmed by the sheer dimensions of the problem, which embrace economics, history, politics, even aesthetics, and of Kovel's amazing ability to psychoanalyze and suggest hypotheses. No psychiatrist myself, I am necessarily cautious though deliberate in my criticism of, for example, his use (surprisingly uncritical) of Freudian psychology which gives his study of white racism a heavily sexual cast unwarranted in light of other studies by Ashley Montague, Oliver Cox, Kenneth Clark, *et al.* He writes that "Modern aversion [aversive racism] stems from anal sadism, while domination [dominative racism] is phallic and oedipal in origin" (p. 85); on the basis of what evidence are we to accept this? Where the monocausal analytical instrument of Freudian sexualism dominates Kovel's hypotheses, greater attention even to the application of Jung's concepts of the impact of individual and collective unconsciousness on individual personality (individual racism) and group behavior (institutional racism) would provide just as credible criteria for an analysis of white racism.

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In an effort to have us understand the basic difference between regional types of racism, such as exist in the North and South, Kovel describes two types of racists—dominative and aversive; but to agree with his theories of the personality determinant of racism, one has to conclude that perhaps there are many varieties of racism, that region is a very poor guide to the distinction, and that certainly the development of personality is influenced by factors besides sex or region, though these are important factors.

Being black, however, I must admit the possibility of not clearly understanding much of the motivation behind "white racism" and Kovel has reinforced this idea by his depth psychoanalysis of the "white mind." In one instance, he says "The id, then, is the referent of blackness within the personality; and the various partial trends within the id, all repressed, make themselves symbolically realized in the world as the forms of blackness embodied in the fantasies of race" (p. 66). It is difficult to simply assert this, however, without an analysis of the archetypal racial factors which are the substance of these fantasies, and such an intimate knowledge of such archetypes must transcend the excellent seminal work of Winthrop Jordan to which the author refers. Without better balance and versatility within the scope of the "psychology of racism," the author will again wander off into such tangential areas as pollution, the military-industrial complex, and the like. All of which leads me to conclude with the publisher that this book is definitely controversial, but that although Kovel has chosen some of the right tools to examine the problem of "white racism," for now, his reach has generally exceeded his grasp.

RONALD WALTERS
Brandeis University

The Dilemma of Accountability in Modern Government: Independence versus Control *edited by Bruce L. R. Smith and D. C. Hague. New York, St. Martin's Press, 1971.—xvii, 391 pp. $15.00.*

If there is one term that I would eliminate from the public administration lexicon it is the word *quasi*. Use of the term *quasi-nongovernmental* to describe the wide variety of disparate phenomena identified and discussed in this volume of essays is a reflection of current ambiguity in thinking about the appropriate role of government and its relationships to new types of institutions which do not fit precisely into the traditional mold. Clarity of analysis and public understanding are not promoted by lump-

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ing under the general heading of quasi-nongovernmental everything from nationalized industries and so called captive corporations on the model of the aerospace corporation to research grants to universities and federal grants-in-aid. The book is the outcome of an Anglo-American conference at Ditchley Park in March 1969 to explore the problems of public accountability and control resulting from the devolution of traditional government functions to private organizations on a contractual basis. The chapters consist principally of papers written for the meeting and two essays, one by Professor Bruce L. R. Smith of Columbia University and the other by Professor Douglas Hague of Manchester University, summarizing the basic themes of the conference from the perspectives of the American and British participants. The papers cover such diverse subjects as contracting for academic research, the university-managed laboratory, government contracting with industry, federal grants-in-aid, and financial accountability to Parliament. As might be expected, the individual papers, with some notable exceptions, appear to be designed primarily to furnish background information for the participants, much of it extremely elementary, rather than to offer fresh insights into the central problem or to suggest new approaches. Alan Pifer, president of the Carnegie Corporation, is correct in stating in his preface that the book is "an incomplete statement of a large subject—a tentative venturing into a new field." In many countries the ministries now play a secondary role. The most challenging and innovative programs are administered by public corporations, autonomous agencies, and private contractors. The implications of this trend for responsible democratic government have not been explored adequately by scholars in the field, except for the important contributions of A. H. Hanson and William Robson on government corporations and Don K. Price and Bruce L. R. Smith on "government by contract." The book is useful because it focuses attention on critical issues of concern to all modern governments.

HAROLD SEIDMAN
Washington, D.C.

Quantitative Ecological Analysis in the Social Sciences *edited by Mattei Dogan and Stein Rokkan. Cambridge, Mass., The M.I.T. Press, 1969.—xv, 607 pp. $25.00.*

This volume, the product of the Evian Symposium sponsored by the International Social Science Council in 1966, is addressed to a complex of issues that has long exercised quantitatively and behaviorally oriented

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political scientists and other students of social and political behavior. At the center of both this complex of issues and the concerns of this volume is the incommensurability of, or even conflict between, analyses based upon sample surveys of individuals, on the one hand, and, on the other, what have been called "geographical-ecological analyses"—analyses based upon data aggregated by administrative or geographical unit. Both have advantages so obvious that it is not necessary to discuss them in detail here. It is sufficient to note that the sample survey permits manipulation of a wider range of variables and may preserve the "wholeness" of individuals, thus permitting more direct behavioral analyses and inferences. The "ecological" mode facilitates the identification of local variation, may make possible studies of considerable time depth, and may encourage the analyst to take into account global variables. Both modes of analysis also have obvious limitations, largely concomitants of virtues which become difficulties when they are pushed beyond their capacities.

This volume is a concerted attempt to recognize and contend with some of the criticisms leveled at ecological analyses, to specify the proper role of ecological analyses, and to define the relationship between individually and ecologically grounded data. We are led here, of course, to the problem of cross-level inference, the discussions of which by the editors and by Alker, Allardt, Valkonen, Linz, and Scheuch are perhaps the most important parts of the book. Alker expands the argument concerning the ecological fallacy initially put forward by W. S. Robinson ("Ecological Correlations and the Behavior of Individuals," *American Sociological Review, XV*, 1950, pp. 351-57) into a typology of fallacies. But it is important to note that this constitutes a basis for a counterattack. Alker labels one of the major types the "individualistic fallacy"—"the confusion of relationships among individuals with those governing aggregates." This is the reverse of the fallacy noted by Robinson many years ago. But even Robinson's ecological fallacy is defended in this volume. Allardt argues in his refreshing contribution that analysts, perhaps, have been too much concerned with the rigor of their method and not sufficiently concerned with the fruitfulness or informative value of their interpretations. He asserts, and I think rightly, that cross-level generalizations are likely to have high informative value—by implication, *higher* informative value than generalizations containing only variables from a single aggregate level. To put it a little differently, the latter are more likely to be trivial. This assertion implies, of course, that both sample survey and aggregate data are required, and although the contributors to this volume defend the "ecological," they do not propose to dispense with the individual. There is a general call for surveys, but "ecologically

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controlled" rather than "atomistic" surveys.

It is not possible within the compass of a brief review to do more than mention some of the contributions and contributors, but there is sufficient space to observe that all are of high quality—at least informative, at best provocative. The volume is a valuable one, and one which seems to be better integrated than most such symposia. The editors have enhanced this integration with lucid introductions, both for the book as a whole and for each of the six sections.

Having given the volume its due for what it is, I shall permit myself to voice some discomfort with the entire work for what it is not. This may not be fair, and the reader should understand that these remarks come from an ecological anthropologist, one whose work has been concerned largely with the place of "primitive" groups in *ecological systems*, that is, systems composed of the total of living organisms and nonliving substances bound together in material exchanges within some demarcated portion of the biosphere.

It may be that my objection can be dismissed as nothing more than quibbling over terminology. I would prefer it if "ecological" were reserved for reference to ecosystems, to their maintenance and to their effects upon the organisms and populations of organisms participating in them. However, it is half a century since the Chicago sociologists began to construct their urban and regional ecology on an analogy to ecosystems and any hope for purity of usage at this late date is idle dreaming. But more is at stake than a cranky objection to the way some people use a word. What is crucial, I think, is that the term "ecological" implies more than aggregation, and more, even, than aggregation by territorial unit. It implies the existence of natural systems on various levels of inclusiveness—ecosystems, societies, communities, organisms. These systems are in some sense integrated wholes, and an ecological explanation, as I understand it, elucidates the behavior or form of a part in terms of its place in the operation—the processes—of such wholes. Such wholes and their integration are notoriously difficult to conceptualize and even more difficult to represent rigorously, although system theory and cybernetics should not be ignored. But one thing is certain. They are not mere aggregates. They are surely not adequately represented by territorially aggregated data, nor are systemic processes sufficiently illuminated by correlations—even cross-level correlations of large numbers of variables. To put this differently, I think an important issue confronting the social sciences is that of holism versus atomism. The aggregation of data does not convert atoms into wholes. Sample survey data, on the one hand, and what political scientists call ecological data, on the other, are both atomic.

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What I miss in this volume is any sense of holism. Admittedly atomism permits more precision than holism at the present time and may continue to do so for a long time to come. But, with Allardt, I believe our explanatory strategies must give weight to interest as well as to rigor. I would respectfully suggest to political scientists that they review biological ecology and some of the work in ecology being done by (ecosystem) geographers and anthropologists. They may find it sloppy but they will find it interesting, and, I hope, suggestive.

ROY A. RAPPAPORT
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Book Notes

The Yugoslav economic reforms and the self-management system are dealt with in quite different ways by two recent books—*Plan and Market in Yugoslav Economic Thought* (New Haven, Yale University Press, 1971; x, 323 pp. $10.00) by Deborah D. Milenkovitch and *Industrial Democracy: Yugoslav Style. The Effect of Decentralization on Organizational Behavior* (New York, The Free Press, 1971; xvii, 297 pp. $9.95) by Ichak Adizes. The Milenkovitch volume addresses itself to the question of the development of Yugoslav economic theory since the inception of economic reforms in the 1950s. By focusing on the debates that have taken place among Yugoslav economists, the author sheds new light on the problems of economic reform in Yugoslavia. Those portions of the account dealing with problems of economic development and the implications of the self-management system for the functioning of the economy are especially useful. The history of the economic reforms is supplied in outline and is interspersed among the theoretical portions of the analysis; this can be a source of confusion to those not familiar with the Yugoslav scene. Over all, Milenkovitch is most successful in clarifying the theoretical issues, while the more practical problems concerning the reasons for the successes and failures of the Yugoslav experiment in market socialism remain something of a mystery. The Adizes volume is a remarkable piece of original research, involving the in-depth study of two enterprises near Belgrade which manufacture similar products but display markedly different approaches to self-management problems. Adizes dispels any doubts that may still exist concerning the fundamental importance of the self-management system of Yugoslavia. The author does raise certain questions concerning the ability of the self-management system to cope with the demands

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of a competitive market economy, however. The detailed analysis of the self-management mechanisms and the vivid descriptions of the effects of the system on the workers and the managers make this book a fascinating contribution to the study of contemporary Yugoslavia.—PAUL SHOUP, *University of Virginia.*

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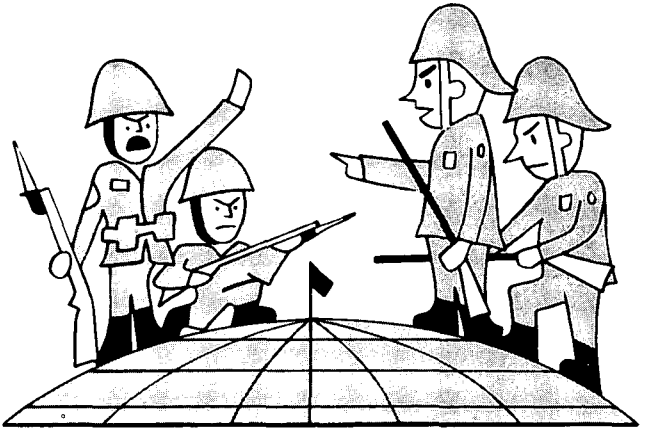

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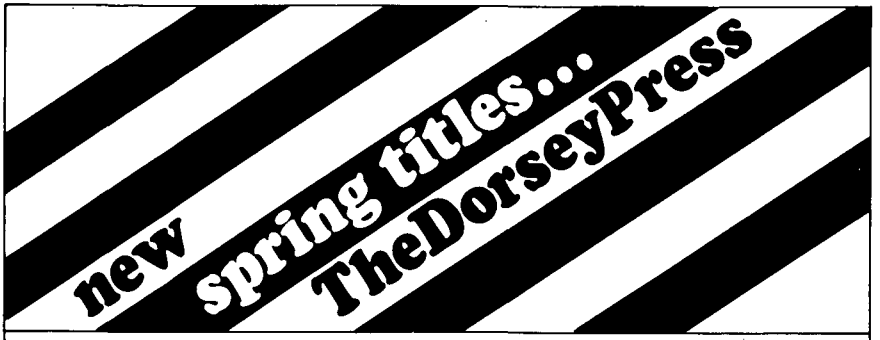

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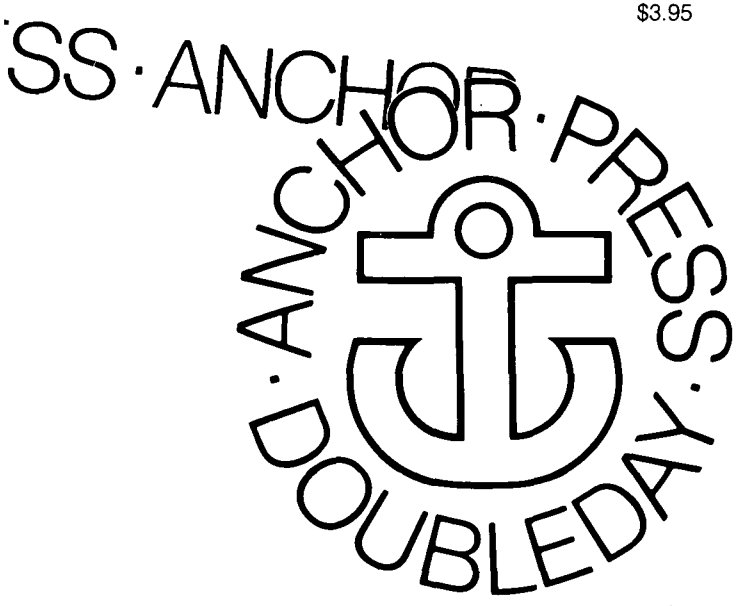

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